An Address
ON
EXPERIMENTAL RESEARCH AND MEDICAL PROGRESS.

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Gentlemen,—The thought which first seeks expression on this occasion is one of sorrow for the loss, through sudden death, of one who was for many years a distinguished teacher in this school. At the end of last summer session the friends and colleagues of Dr. Radcliffe Crocker expected with a reasonable degree of confidence that he would be present with us to-day to take part in the inauguration of another Annus Medicus. It has been otherwise ordained. There remain with us the memory of an able and amiable fellow worker and the grateful recollection of a long record of much good work well done.

The next sentiment is one of congratulation to those who have just received the tokens of their triumphs. May these rewards prove harbingers of future success in the sterner battle of professional life. I trust that I shall not be thought guilty of dropping *aliquid amari* into the cup of happiness if I venture to caution those who are the victors of to-day not to regard the distinctions which they have just attained as ends in themselves, but rather to consider them as incentives to a higher purpose. The successes which we celebrate to-day are indications of ability, of industry, and of steadfastness, but for complete success in life it is not enough to be apt in acquiring knowledge or to possess a retentive memory, or to have facility in written or verbal expression. A man may possess these and yet lack many of those qualities which are requisite to ensure success in practical affairs. Mere cleverness is not enough, nor industry, nor zeal. Character is also needed, and all that this term connotes—thoughtfulness, sympathy, courtesy, and culture. And by culture I do not mean what has been scoffingly described as a smattering of Greek and Latin, but that higher discipline so finely described by Matthew Arnold as

> a pursuit of our total perfection by means of getting to know, on all matters which most concern us, the best which has been thought and said in the world; and through this knowledge, turning a stream of fresh and free thought upon our stock notions and habits.

The proverb runs "Knowledge is power"; but this is not always or necessarily true. Knowledge is one of the elements and conditions of power; to be effective it must be directed and controlled by wisdom. There are many learned men in all professions who, for some reason or another, do not succeed in the practical departments of their calling. It is not easy—sometimes it is not possible—to say why this is so. Sometimes the fault is in external circumstances; more often, perhaps, it is some inherent personal failing or defect. Conspicuous inaptitude for practical art is scarcely possible nowadays. Every student, before obtaining a medical degree or diploma, must go through a prescribed curriculum of study and practical training, and must pass certain statutory examinations as to his knowledge and skill. This was not always so. Until the intellectual awakening of the sixteenth century thought was largely scholastic and trammelled by ecclesiasticism, and professional skill often manifested itself in dialectics rather than in action. Arnald of Villanova, one of the chief medical luminaries of the thirteenth century, relates that he knew an excellent professor of natural science, learned in medical theory, who could not treat the simplest ailment. This is an extreme type, which is now extinct. As the antithesis of the learned but inexpert man there is in all professions the so-called "practical man," whose merits are generally overrated in popular estimation, and whose lack of theoretical knowledge is often imputed to him for righteousness. Nor can it be denied that not infrequently he does attain a certain level of success, but he has narrow limitations. He deals in particulars, is often quick to observe similarities, and, in the treatment of disease, concerns himself with symptoms without troubling to investigate the physical conditions upon which these symptoms may depend. Science arises from the discovery of Identity or Unity amid Diversity of Manifestation, and it is precisely here that the practical man so often fails. He appreciates agreements and similarities, but fails to discriminate differences. Such men are not altogether without illumination, but the light within them is what Jowett has called "a luminous mist." They see things at false distances and in erroneous proportions. In the long run, it will be found that the sanest and safest guide is one whose practical training is based upon sound and sufficient learning.

We are assembled to-day not merely to distribute prizes to those who have already made some progress in their medical study, but we are more particularly gathered together to welcome those who are now beginning the study of medicine. I congratulate all such upon the choice they have made of a career. From the days of Homer to our own times much has been written in praise of the physician and of the profession of medicine, and not a little has been said in dispraise and disparagement. But, take it all in all, I maintain there is no secular calling so philanthropic in its aim, so refining in its influence, and so beneficent in its operation as the practice of medicine. Like Mercy, it is twice blessed: it blesses both patient and practitioner. Doubtless most of those who enter into the medical profession do so with the ultimate object of earning a livelihood, but in the study and pursuit of medicine personal interests gradually become merged in an altruistic Hedonism—the good of others; and there is, I believe, no pursuit which so surely humanizes a man and calls forth his better self, or one in which average ability and good character more certainly meet with reasonable recompense. But, paradoxical as the statement may be, even worldly success is generally most assuredly attained when it is not pursued as the chief end and aim. Medicine is only worthily served when it is practised as a cult in the devout spirit of worship. It must be served loyally and for itself.

It has been said that the criterion of the professional spirit, in contradistinction to the commercial and industrial spirit, is this:—the professional man thinks first and chiefly of the nature and the merit of his work, the commercial man thinks most of the amount of his wage. Nevertheless, one must live, and most of those who enter the medical profession must live by what they can earn. Here, if anywhere, the labourer is worthy of his hire. For some of those who enter the medical profession it may be well that they should possess financial resources sufficient to relieve them of the carking cares of earning their daily bread and of making provision for future contingencies when work is no longer possible. But, for some at least, it is better that they should lack these pecuniary aids and have to rely upon their own spiritual and intellectual resources. Thousands of medical men have lived happy, useful, and honoured lives, and not a few have risen to great eminence and honour who have had to start practice with no other equipment than their technical training and the prayers and good wishes of their friends.

Those who are now beginning medical study may at the outset be perplexed and discouraged, as I was myself, by the new method of study and by the new order of ideas to which they are introduced. Heretofore the education of the student has been chiefly literary, based upon pedagogic authority and the dictatorship of books. Henceforth both the character and method of study will be altered. The influence of authority will still obtain; much knowledge must be taken on trust, but it is a critical trust. Whatever is taught here is based upon a knowledge which may have been the accumulation of centuries, or it may be knowledge recently acquired, but it is knowledge which in due time and place is capable of verification and demonstration. Though this knowledge is based upon authority, it is the authority of persons who have examined it with care and attention, and who have been competent to form a sound opinion upon it. In later life most of us, as Cornewall Lewis has remarked in his essay on Authority, must accept much of our knowledge on the trust of *our*

own authority. We have gone through a process of thought and investigation, and have arrived at a given conclusion, but the reasons of that conclusion have passed out of memory. We hold these opinions rather upon the recollection of having once ascertained them to be well-grounded than from a present perception of these grounds. If this were not so we should, as Locke observes, be perpetually floating about in doubt, and be at the mercy of those who have a readier or more retentive memory, or who have happened to master the arguments on one side of the question only. Meanwhile, and throughout the greater part of the time you spend here in pupilage, you must depend to a very large degree upon the authority of your teachers and of accredited textbooks, but there will never be lacking means of testing the accuracy and scientific value of this teaching by appeals to the evidence of positive data.

The primary purpose of medicine is the relief of human suffering and the cure of disease, but it has a higher ulterior motive, inasmuch as it aspires to the more important office of endeavouring to prevent disease and to remove or antagonize the causes and conditions which induce disease either in the individual or in the community.

The growth of medical knowledge has been slow and subject to cyclical variations, sometimes to retrogression, sometimes almost to eclipse. But it *has* grown, "creeping on from point to point." The history of medicine furnishes many illustrations of this. In human anatomy, for example, it would at first sight seem to be easy with due diligence to ascertain and correctly describe the structure of the human frame, but it took many centuries of practical observation to reach even an approximate degree of accuracy. Aristotle, Hippocrates, Praxagoras, and other physicians of the Hellenic and the Alexandrian schools, studied anatomy, and yet the records of their observations contain many errors both of observation and of reasoning. Even Galen, who was considered the infallible oracle for fourteen centuries, and who was the first to study seriously the science of anatomy and made many discoveries therein, also promulgated many errors. But notwithstanding the impulse which Thaddeus and Mundinus gave to the study of anatomy at the school of Bologna in the thirteenth century, the authority of Galen was so firmly established that, when Vesalius in the middle of the sixteenth century published his famous work on human anatomy, in which he corrected many of Galen's errors, there were not wanting anatomists who impeached the accuracy of Vesalius, or, when this was no longer possible, affirmed that the human organization had undergone structural change since Galen's time. Now, there is not a first year's student who will not be able daily to verify every statement made by his teacher or contained in his textbook. The progress of physiology has been even slower than that of anatomy. Indeed, scientific physiology is almost entirely the work of our own time. Forty years ago physiology was taught in University College by the greatest exponent of the day—I mean the illustrious Sharpey; but the physiology which he taught was almost entirely didactic and expository. There was practically no experimentation and scarcely any attempt at practical demonstration. Now physiology is pre-eminently experimental, and the splendid position which it has attained amongst the natural sciences is the result of the employment of the experimental method.

The course of medical education is progressive in complexity. In the earlier studies the method is authoritative and demonstrative, but in the final studies of medicine and surgery the method is more conjectural and ratiocinative. Here, exact observation, well trained senses and correct reasoning are the critical apparatus required. The problems to be solved are more complex and more obscure than those of anatomy and physiology. The difficulties of investigation are greater, the phenomena to be observed more difficult of interpretation, and the means of verification are fewer. "Art is long," says Hippocrates, "the occasion fleeting, judgement difficult." The investigation of disease involves the study of a compound and aggregate of many phenomena, some of which are common to many different physical states. A pathognomonic sign is often wanting. Moreover, some of the phenomena expressed in a diseased state may be essential, some accidental, and some mere concomitants, and these must all be differentiated and their relative values separately determined. In clinical medicine it is rarely possible to conduct an exact experimental inquiry. As Mill has pointed out, there is this essential difference between observation and experiment: in observation we *find* our instance in Nature; in experiment we *make it* by an artificial arrangement of circumstances. In clinical medicine we can seldom isolate the phenomenon under observation by an artificial arrangement of circumstances. Hence the growth of medical knowledge has been slow and halting. Centuries of observation by experienced workers have been needed to raise practical medicine to its present high level. But these observations have, for the most part, only improved the art of medicine and have done comparatively little for the advancement of its science. The science of medicine has mainly been advanced by the researches of those who have not been directly engaged in the treatment of disease. When new knowledge has been discovered by these means it has been received by competent judges, and, by the influence of their authority, it has been accredited and diffused. John Henry Newman in his discourse on the Idea of a University, has portrayed the qualifications which are required for the advancement of knowledge as distinguished from those which are needed in the dissemination of knowledge. He says:

> To discover and to teach are distinct functions; they are also distinct gifts and are not commonly united in the same persons. While teaching involves external engagements, the natural home for experiment and speculation is retirement.

Failure to appreciate the difference between the dissemination of knowledge and the advance of knowledge has given rise to much confusion of thought and not a little waste of endeavour. Perhaps the crudest manifestation of this confusion is the belief, real or feigned, by some persons, that inquisitorial experiments are performed in hospitals on men, or on animals, or both. Less crude—nay, even creditable in a sense—is the notion that, by establishing sanatoriums or endowing special hospitals for the treatment of particular classes of general diseases, it is possible to solve the problem of the nature and origin of these diseases and to hasten the discovery of the means of prevention and cure. Logically, this assumption is false; economically, it is wasteful. It is based on a misapprehension of the function of hospitals and the methods by which scientific discoveries are made. Far be it from me to discourage the most generous pecuniary support of hospitals. Money is greatly needed for their maintenance, and will be needed in increasing proportions if hospitals are to avail themselves of the resources which scientific research is continually placing at their disposal. The great desideratum of our day is adequate endowment of biological and pathological research as the surest and most humane way of discovering the nature and cause of disease and the means of its prevention.

Clinical observation only deals with disease when and as it manifests itself in man and animals; it throws but little light upon the causes of disease or upon the intimate processes which constitute the pathological state. Supremely useful as hospitals are for the purpose of treating the sick by means of the best skill and knowledge of the time, and as schools for the training of successive generations of medical practitioners for the service of the community, it is nevertheless true that most of the capital discoveries of scientific medicine have been made outside hospitals, and very often apart from clinical observation altogether. Among the most remarkable of these discoveries I may mention those relating to malaria, Malta fever, yellow fever, and many other diseases which have baffled the best clinicians for centuries. In surgery, too, it may be said that the scientific basis of the modern aseptic method has been established not by clinical observation but by experimental research. Another simple but striking instance supporting my thesis is afforded by the history of the use of the ligature for the arrest of haemorrhage from wounded arteries. Nothing, it might seem, can be more obvious than to tie a bleeding artery. The records of surgery inform us that the ligature had been so employed more or less systematically by surgeons for over two thousand years; but, simple as the procedure may seem, the true method of tying an artery was not established until the beginning of the last century. Ambroise Paré, about the year 1560,

recommended tying bleeding arteries in primary amputation instead of applying the cauteries then in vogue; but his plan of securing the vessel was so difficult, and was attended with so much subsequent risk, that most surgeons condemned it. An English surgeon, Alexander Read, in lectures which he delivered at the Barber-Surgeons' Hall, about the year 1630, condemned Paré's practice as "a troublesome and dangerous toy, as he shall finde who shall go to make trial of it."

For more than two centuries surgeons were divided in opinion as to the value of the ligature, and even those who did employ it were afraid of injuring the arterial tunics by applying the ligature too tightly. Various contrivances were used to avoid damaging the coats of the vessel. Thick and broad threads, tapes, ribbons, cylinders of cork, wood or linen, reserved ligatures, and other expedients were employed to stop bleeding without injuring the walls of the vessel. Recurrent and secondary haemorrhages were frequent. Dr. J. F. D. Jones at the beginning of last century, by a series of ingenious experiments on dogs and horses, proved that the endeavour of surgeons to apply the ligature without injuring the arterial tunics was precisely the cause of the failure of the ligature to permanently arrest bleeding. Jones demonstrated that the true method was to employ a small, firm ligature drawn sufficiently tightly to cut through the inner and middle coats without rupturing the tougher and external coat. It is instructive, in view of the modern outcry against the experimental method, to read Jones's *Apologia*, bearing in mind that his experiments were performed before the days of anaesthetics and painless operations.

He regrets (he writes) the necessity of obtaining this important knowledge by the sacrifice of brutes. But when we remember the incessant scourge of war which has followed man through all the ages of his history, not to mention the consequences of accident and disease, it is not too much to assert that thousands might have been, and may still be, saved by the knowledge of these subjects; which can only be directly obtained by experiments on brutes; indirectly and very slowly by observations on the injured arteries of men; and even this cannot be made until he has fallen a sacrifice to the want of assistance or to the imperfect knowledge of the surgeon.

Every practical surgeon will bear testimony that, as the result of Jones's experiments, tens of thousands of lives have been saved.

I have lingered about this question because you, as future practitioners of medicine, will have to form an opinion on the justifiability of experiments upon living animals for the purposes of scientific research, and if you are satisfied that these experiments are justifiable with proper safeguards, you will be called upon to defend the practice and to justify your opinion. To do this intelligently it is necessary not only that you should be acquainted with the arguments for the defence, but that you should also make yourself master of the arguments on the other side. It is not enough to ascribe the opposition of the public to ignorance or fanaticism. There may be either or both, but objection can only be overcome and acquiescence gained by enlightening and educating the public mind. It should be remembered that the mass of a nation must be convinced of the value of a general principle which is being carried out, else what we might judge the most salutary change will be ineffectual. Ritchie, in discussing the Rights of Minorities, has remarked that on matters of public health only the scientific expert can, in the first instance, form a sound judgement. In democratic societies there is often a distrust of scientific opinion. The remedy is not despotism, but public enlightenment, and the scientific specialist is bound, therefore, by patriotism, as well as in the interests of his own science, to lend what aid he can to the popularization of science from which he is too apt to recoil. It is the sole antidote to ignorance or pseudo-science. Those whom science neglects, fanaticism and quackery will claim for their own.

Gentlemen, I have said nothing about your scheme of work or of the manner of your lives. These I confidently leave to the help and guidance of your teachers and to the moral and religious impulses which you bring with you. As touching medical ethics, I believe the Law and the Prophets are summed up in the vow prescribed by the Hippocratic oath: "In purity and in holiness I will pass my life and practise my art." You are heirs to a great inheritance. Into your care and keeping will be entrusted the health, the happiness, and the lives of many of your fellow-creatures. So acquit yourselves that at the trial you may be found not unequal to your responsibilities.

Remarks
ON
COMPLEMENT FIXATION
AS A METHOD OF DIAGNOSIS APPLIED TO SYPHILIS AND GENERAL PARALYSIS: THE WASSERMANN REACTION.

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We have investigated the method in a considerable number of cases of syphilis and general paralysis, and in presenting our results we take advantage of the opportunity briefly to review the vexed question of the nature and specificity of the reaction as applied to these diseases. Whether or not the current view that it is not "a true antigen antibody" reaction, and "not due to the interaction of antibody or syphilitic antigen in the usual sense of the word and has no relation with the *Spirochaeta pallida*" (Mott[47 74]), be correct, the test cannot be regarded otherwise than as a highly reliable and specific one for the disease caused by that organism.

Into the technique of the method and the facts on which it is founded there is no necessity to enter in detail. The introduction of modifications[73] of the original method devised by Wassermann and his co-workers[8] for showing the presence in syphilitic serums of substances induced or pathologically increased by the disease, and of new[73] methods for effecting the same object, is so rapid as to make it difficult to become practically acquainted with them all. It seems probable that some of these may prove more suitable for diagnosis than the original. Our experience is based chiefly on the latter, with but slight modifications; and so far we have not found sufficient reason for departing from it. A difficulty involved in the original technique described by Wassermann,[8] by the necessity for obtaining a comparatively large quantity of serum, is satisfactorily obviated by diminishing the amounts of the reagents used. If 0.1 c.cm. of each of the properly diluted reacting substances be taken, and tubes used made from ¼-in. glass tubing, in which the ultimate total quantity—0.55 c cm.—can easily be thoroughly mixed by rotation between the palms, the results are as accurate as when large quantities are employed, and the gain in convenience is great. The handy 1 c.cm. pipettes graduated in one-tenths and one-hundredths can then be employed for complement, extract, and saline; for the serums and cerebro-spinal fluid, ordinary Wright's pipettes, graduated in 0.05 and 0.1 c.cm. by means of mercury, conveniently measured by Wright's small pipettes for measuring 0.005 c.cm. (0.01359 gram) of mercury, are the best.[71] A convenient means of marking these graduations is a blue grease pencil, which is pressed against the glass while hot; such a mark will stand repeated sterilization.

We found it well to test the complement each time to ascertain the minimum haemolytic dose; thus:

Guinea Pig's Serum (1 in 10).	Sensitized Sheep's Corpuscle (5 per cent. Suspension of Blood).	Saline (0.9 per cent.).	Usual Result.
0.05	0.1	to 0.55 c cm.	+
0.04	0.1	,,	+
0.03	0.1	,,	+
0.02	0.1	,,	80-95 per cent.
0.01	0.1	,,	30-80 per cent.
—	0.1	,,	No haemolysis

The corpuscles are sensitized by the addition of rabbits' immune serum in the proportion of 1 to 500 (about five times the haemolyzing dose) to a 5 per cent. suspension of washed defibrinated blood. The corpuscles are, after incubation, rewashed, and again suspended in normal saline. Complement is obtained easily by bleeding the ear veins of one or more guinea-pigs, after washing with hot water.

With regard to antigen our experience has decidedly pointed to the watery extract of a liver rich in spirochaetes as the best. When testing cerebro-spinal fluids we thought that an alcoholic extract answered as well as a watery one. The use of two extracts in doubtful cases is sometimes of assistance. The following is a convenient and satisfactory method of conducting a test:

Number of Tube.	Normal Serum (1 in 5).	Syphilitic Serum (1 in 5).	Suspect Serum (1 in 5).	Syphilitic Liver Extract (1 in 7).	Complement (1 in 10).	Sensitized Corpuscles (5 per cent. Blood).	(Saline (0.9 per cent.).	Result (+ = Haemolysis).
	c.cm.	c.cm.	c.cm.	c.cm.	c.cm.	c.cm.	c.cm.	
1	0.1	—	—	0.1	0.1	0.1	to 0.55	+
2	0.1	—	—	,,	0.07	0.1	,,	+
3	—	0.1	—	,,	0 1	,,	,,	0
4	—	—	0.1	,,	,,	,,	,,	
5	—	—	0.1	,,	0.07	,,	,,	
6	0.1	—	—	—	,,	,,	,,	+
7	—	0.1	—	—	,,	,,	,,	+
8	—	—	0.1	—	,,	,,	,,	+
9	—	—	—	0.1	0.1	,,	,,	+
10	—	—	—	,,	0.07	,,	,,	+

The complete reaction is considered to be given when 0.1 c.cm. of complement 1 in 10 (about four haemolytic doses) is "deviated." Indications of great value are, however, often obtained by reducing the complement or by increasing the extract or serum, especially the former. Readings taken at half, one, and two hours in the incubator, as well as the final one after eighteen hours in the ice chest, are sometimes of value. It need hardly be insisted upon that the greatest cleanliness and attention to details is essential for satisfactory and consistent results. The glassware should be sterilized before use and washed as soon as possible afterwards.

In the interpretation of results, apart from the general question of the specificity of the reaction, we have found some difficulty in ascertaining the exact standard adopted by the various workers. If the term "complement binding" be taken to represent accurately what occurs, then the method of estimating as accurately as possible the actual number of haemolytic doses of complement fixed, as used by McKenzie,[35] seems rational and useful. A considerable amount of European and American literature is not available in New South Wales, and we have possibly missed some papers giving more definite information on the subject. Our practice is that if there be a complete, or practically complete, absence of haemolysis and the controls be correct, we make a positive diagnosis of syphilis. In suspected cases, if haemolysis be partial or complete, the further tests above indicated are carried out, using perhaps another extract, and a judgement formed, in accordance with the results, as to the presence in modified degree of the reaction. In such cases the diagnosis is qualified. Under similar circumstances Bruck and Stern[18] recommend varying the amount of antigen and of serum. In the cases tabled, only those which gave the full reaction are recorded as positive. In most of our investigations two extracts were used at the same time, one acting as a control of the other. We are much impressed with the necessity for using full controls. The serums and cerebro-spinal fluids seldom bind alone to any extent; the extract should be standardized to bind alone not more than one haemolytic dose of complement. If the duration of the test be long, as when a large number of tubes are put up at once, the complement tends to deteriorate, and this must be allowed for.

The following are the results given by the serum and cerebro-spinal fluid of the cases of general paralysis which we investigated:

Number of Cases of General Paralysis.	Serum.			Cerebro-spinal Fluid.		
	Number tested.	Result.	Percentage of Positive Results.	Number tested.	Result.	Percentage of Positive Results.
57	56	56+ 0−	100	57	56+ 1−	98.2

In connexion with these cases we did a number of controls. The results of these are shown below:

Disease.	Number Tested.	Serum.		Cerebro-Spinal Fluid.	
		Results.	Percentage Negative Results.	Results.	Percentage Negative Results.
Dementia praecox	3	0+ 3−	100	0+ 3−	100
Epilepsy ...	2	0+ 2−	,,	0+ 2−	,,
Imbecility	1	0+ 1−	,,	0+ 1−	,,
Mania del.	2	0+ 2−	,,	0+ 2−	,,

Cases of syphilis or definitely suspected to be such, also used as controls:

Disease.	No. of Cases.	Serum.	
		Results.	Percentage Positive Results.
Congenital syphilis	2	2+	100
Primary syphilis	2	2+	100
Secondary syphilis with symptoms	34	33+ 1−	97
Tertiary syphilis with symptoms...	3	3+	100

In addition to the above, which represent the cases done with the definite object of adding to the sum of experience which is being accumulated with regard to the efficacy of the test in known cases of syphilis and parasyphilis, we have made a very large number of examinations of serums of cases for diagnosis. The results, while they fully confirm our experience of the high clinical value of the test, do not, on account of the doubtful nature of the clinical diagnosis in many cases, lend themselves to the purpose of comparative results.

RELIABILITY OF THE TEST FOR DIAGNOSIS.

For the practitioner who has constantly to deal with these syphilitic cases the question of the reliability of the test is one of the first importance. Can he give definite advice, the correctness of which is a matter of the most vital and far-reaching importance to his patient, based on the result of the test? In the first place, it must be definitely borne in mind that there are limitations as well as great possibilities in the test. Only harm can result from the idea that it is infallible. It is not yet an "open sesame" to every case suspected of syphilis, but it is a most valuable aid in many cases of difficulty.

Only the result of a vast number of examinations of syphilitic and non-syphilitic persons can settle the question of the absolute value of the reaction, and probably the last word in technique and method has not yet been

spoken. But sufficient work has been done to enable a fairly definite opinion to be arrived at, and extensive series of statistics by different workers are almost daily becoming available.

As regards a positive finding there can be little doubt: the experience of almost all workers seems to show that with proper technique and control a positive reaction means syphilis. Though in a few diseases, such as leprosy,[10 28] trypanosomiasis,[13 49 55 56 57 58 1] noma,[59] yaws,[61 18] etc., some works report positive findings, the reaction in these diseases, though of great theoretical importance, is not likely to affect the question of diagnosis often; while, apart from an occasional case, such as that given by McKenzie,[35] where it must be considered doubtful if syphilis can be excluded, a positive reaction in non-syphilitic cases, such as scarlet fever, is usually either slight or based on doubtful technique.

With regard to the most important question of the value of a negative reaction, we have been given some indications for the formation of an opinion in the course of our own work, and we have also endeavoured to make use of the work, and to ascertain the opinion of those best qualified to judge. The outstanding fact seems to be that with improved technique the number of definitely syphilitic cases not giving a reaction becomes fewer and fewer. Undoubtedly, proved reliability of technique is essential in any one presuming to offer an independent opinion on this question.

We have touched on the indication given in cases of doubt by altering the quantities of complement, extract, etc., in the test. It is undoubtedly a remarkable tribute to the acumen of Wassermann and his colleagues that, for the test, the same substances, prepared in the same way, and used in the same quantities advised originally by them are found most generally useful to-day. It seems to us, however, that in some cases the deviation of somewhat less complement than that given in Wassermann's original paper will often furnish information of definite value. We have found also that cases giving a doubtful reaction with one extract of proved efficiency may occasionally give a positive reaction with another of no obviously greater power. The absence of the reaction in undoubted non-syphilitics, or in cerebro-spinal fluid in cases definitely not paralytic has, in our (of course, limited) experience, greatly struck us. In no normal person have we been able to obtain with any extract a reaction which could be thought syphilitic, in the conditions of the experiment given by Wassermann. The normal cases which we have used have varied to the extent of not more than 2-minim doses of complement. A curious fact was noted that the normal cases which gave the greatest deviation with syphilitic extract gave an unusually large deviation also with typhoid, with plague, and with diphtheroid "extracts."

Though of prime importance in relation to this question, it is perhaps unnecessary to consider here in detail the statistics concerning the percentage of positive reactions in different stages of the disease, and after treatment given by the recent workers on the subject.[27 26 35 21]

A case seriously suspected of syphilis, or in whom a certain diagnosis is important, should be examined with varying amount of complement and with different extracts, well controlled with known normal and syphilitic serums. The results must, of course, be judged with due regard to the effect of any treatment the individual may have undergone, to the stage of the disease, and to the presence or absence of active morbid changes. Undoubtedly in secondary or in active tertiary untreated syphilis the value of the reaction must very nearly approach the Widal reaction for typhoid[39] done under the best conditions; while with regard to general paralysis it would almost seem that in a case whose cerebro-spinal fluid and serum have been carefully and repeatedly examined with negative results, a clinical diagnosis of general paralysis must be seriously questioned.

In addition to the matter of diagnosis, a question of the highest importance is the bearing of the reaction on the prognosis in all its aspects—for example, in the duration of treatment, in the question of marriage, and of liability to late or remote effects of the disease. On all these guidance is much needed, and will be sought from the information afforded by the reaction. It must be acknowledged that this guidance is not as yet to be obtained in any full degree, but it may confidently be expected that when the results of investigations extending over sufficient time, and including sufficient observations directly bearing on these questions, are available, our knowledge on these most important matters will be enormously increased.

Some Theoretical Considerations.

The discovery of the property in the cerebro-spinal fluid in general paralysis and tabes of specific complement binding, and its association with the presence therein of new substances, or increase of substances, originally present in slighter degree, has led to much investigation and conjecture as to the nature of these substances and their relation to the Wassermann reaction. In the course of our practical investigations with the Wassermann test we made some observations which bear in some degree on the subject, and these we think it well to record.

The fact that a degenerative process, associated with breaking-down of nervous elements in the central nervous system, is an essential feature in general paralysis and in tabes, suggests the possibility that in other conditions where extensive destruction of neurones is taking place the products of degeneration may, in view of the supposed chemical explanation of the phenomenon, give similar complement deviation to that given in general paralysis or tabes. To investigate this we obtained the cerebro-spinal fluid and serum of 3 cases of extensive vascular cerebral lesions leading to more or less complete hemiplegia, and investigated these for antibodies. The lesions had occurred three, twelve, and twenty-four weeks previously. No evidence whatever was obtained of specific complement deviation. The total albumen content, as shown by precipitation with salicyl sulphonic acid, was the same as that of a normal cerebro-spinal fluid obtained at the same time, and was one-tenth the amount of that in the cerebro-spinal fluid of a case of general paralysis also taken at the same time as a control. The deviation test showed that the three hemiplegic fluids, and the normal fluid did not, even in large amounts, influence the complement to the extent of one minimum haemolytic dose above the amount absorbed by the substances separately, while in the general paralytic at least twelve extra doses were neutralized.

In a case of tabo-paresis investigated at the same time the albumen content was scarcely more, as shown by centrifuging, than in the hemiplegics, though it characteristically separated in larger flakes when allowed to deposit slowly. The Wassermann reaction, however, in the tabetic was well marked. This observation is of interest in connexion with that (vide infra) of the absence of complement-binding property in the concentrated alcoholic precipitate of normal fluid as compared with one of similar concentration from the fluid of a case of general paralysis, and points to the responsibility of other factors than a general increase in proteins for the specific complement-binding reaction.

We have investigated the question whether a specific deviating substance could be thrown down from the cerebro-spinal fluid in general paralysis by certain chemical agents. Our choice of these was conditioned by their convenience with regard to the separation of the precipitate and supernatant fluid, and to their own intrinsic effect on the test rather than by their importance as specific precipitants, in accordance with the current views as to the nature and import of the bodies contained in the cerebro-spinal fluid.

In view of recent investigations into the nature and reactions of the globulins,[41] it would appear premature to accept unreservedly the at present widely held belief[69 47 33 32 31] that the increase in protein content is due to globulins, and that these globulins are definitely the source of the complement-deviating reaction given by these fluids.

At the outset we assured ourselves that in the cases we were working on there was a marked difference between the albuminous content of the general paralytic fluids and the normal fluids. There is no room for doubt that, apart from acute or subacute diseases, a heavy flocculent precipitate occurring in a cerebro-spinal fluid on the addition of a general albumen precipitant is very strong evidence that the fluid was derived from a case of general paralysis or tabes. It would seem that, apart from any specific reagents for precipitating globulins, or other supposed characteristic substances in the fluid,[31 43 33 69] the simple test of complete precipitation of the albuminous contents

with any suitable reagent—for example, salicyl-sulphonic acid—will give almost equally certain diagnostic information. We estimated approximately the albuminous content of a number of cerebro-spinal fluids in the following manner. A large number of specimens were obtained at the one time. A corresponding number of small calibre tubes made from the same length of glass tubing were prepared, and into each 0.3 c.cm. of a cerebro-spinal fluid was pipetted. Three drops of salicyl-sulphonic acid were added to each tube, and the whole of the tubes were immediately centrifuged together for two minutes on the same centrifuge. At the end of this time the supernatant fluid was carefully pipetted off and discarded, and the deposits were carefully measured in a fine pipette. The results in the large number of cases examined was a definite differentiation of general paralytic from all other fluids examined. One case only of tabes was examined, that referred to above.

Three reagents were used to obtain precipitates: alcohol (80 per cent.), phenol (3 per cent. solution), and salicyl-sulphonic acid. The alcohol was added to the fluid in the proportion of 4 parts of alcohol to 1 part of fluid. The precipitate obtained—which consisted of the total precipitable contents of the fluid, as tested, after separation of the precipitate, by salicyl-sulphonic acid—was well washed and rediluted or suspended in normal saline to the original bulk of the fluid. The supernatant fluid was freed from the alcohol at a low temperature under an exhaust. Both supernatant fluid and the precipitate were then tested as original cerebro-spinal fluids. The precipitate from the normal fluid was dissolved in about six times less saline than that from the general paralytic fluid; treated in this way, the total precipitable material in the normal and the general paralytic dilutions were made approximately equal. The phenol precipitate obtained by adding 3 per cent. phenol to the cerebro-spinal fluid was treated in a similar manner. The phenol supernatant fluid contained 3 per cent., of course, phenol; the action of this was therefore examined by controls. According to Wassermann's direction, 0.5 per cent. phenol is added to the cerebro-spinal fluid to be tested before inactivation. His object in directing this we could not ascertain. In one experiment of our own, involving a large number of cerebro-spinal fluids, these were tested in duplicate with and without the phenol. The results were most definitely more satisfactory when this was added—that is, the cerebro-spinal fluid with phenol absorbed the complement more completely without the controls being affected in such a way as to influence the results of the test.

This is in accord with the action of phenol on complement when tested with and without syphilitic antigen in quantities insufficient to cause physical haemolysis. The salicyl-sulphonic acid precipitate was treated similarly, the precipitate, well washed, being suspended in 0.9 per cent. saline; the excess of the reagent in the supernatant fluid so interfered with the reaction that it could not be used.

The results of a large series of examinations can be briefly summarized as follows:

Alcohol.—The alcoholic precipitate from the cerebro-spinal fluid of cases of general paralysis had very high complement absorbing properties, but not of a specific nature, that is, the addition of syphilitic extract did not increase it. The precipitate from normal fluids fixed complement to a very slight extent indeed, even when used concentrated as described above. The supernatant fluid of normal and general paralytic cerebro-spinal fluid did not bind even in large amounts.

Phenol.—The results with the phenol precipitate were similar, but the precipitate had very much less complement fixing property than the alcoholic. There was no precipitate with normal fluids. The supernatant fluid had some slight complement fixing properties.

Salicyl-sulphonic Acid.—The precipitate exhibited no complement fixation properties when used in large amounts with or without extract.

As the complement deviating power of the cerebro-spinal fluid might have depended on two factors, one of which was precipitated and the other remained in the supernatant fluid, we examined the action of a readmixture of the washed precipitate and the supernatant fluid with regard to specific complement binding action; our results, as far as they went, tended to show that the precipitate and supernatant fluid, when recombined, possess no specific deviating power.

It would, therefore, appear that the chemical agents used, where they did not destroy the whole binding action, destroyed its specific nature—that is to say, that as regards these precipitates, binding occurs to an equal extent with or without the presence of syphilitic virus or other antigen, which, with the unaltered fluid, brought about an enormously increased complement inhibition. Whether the explanation of this is that a specific binding substance is destroyed by the chemical agents used, or whether it is that a complex and easily disturbed relation existing between substances which occur to a larger extent in the cerebro-spinal fluid in cases of general paralysis than in others is broken down, we are unable to offer an opinion.

Recently Noguchi and Bauer have succeeded in precipitating the specific deviating bodies. Such a result cannot fail to have an important bearing on the elucidation of the theoretical aspect of the question.

It would almost appear that there may be two properties inhibitory to complement action possessed by general paralytic cerebro-spinal fluid — one a general one differentiating general paralytic from normal fluids but not specifically syphilitic, and another more definitely and specifically syphilitic and more easily destroyed.

Brain Extract as Antigen.—We tested extracts of normal and paralytic brains for antigen and for antibody. The extracts were prepared in an exactly similar manner to the liver extract. The general paralytic brain extract contained antibody, but not to a large extent. The antibody reaction with syphilitic liver extract as antigen persisted, though in greatly diminished degree, for about six weeks. The normal brain was not found to contain nor to develop any specific antibody reaction, as compared with the paralytic, on keeping.

Antigen.—No definite superiority in antigen action by the extract of paralytic over that of normal brain was found, though on some occasions we thought that there was perhaps slightly more specific complement binding with the paralytic than with the normal brain. Both extracts gave some specific binding with syphilitic serums and the cerebro-spinal fluid of cases of general paralysis, though in a very much slighter degree than the liver extract. One peculiar fact was noticed—that very much more definite and positive results were obtained when using cerebro-spinal fluid than when using serums. Serums which gave an extremely marked reaction with liver extract, gave in the same quantities hardly any result with the brain extracts, while cerebro-spinal fluid whose complement binding power with liver extract was known to be not so great as that of the serums, gave a complete reaction with the brain extract.

Antigen in Cerebro-spinal Fluid.—We examined specimens of cerebro-spinal fluid, which gave strong antibody reaction, for antigen. We did not succeed in demonstrating the presence of antigen.

In a recent paper in the British Medical Journal by Mott references are made to the reaction in the blood and cerebro-spinal fluid of cases of syphilis and parasyphilis. It seems that in active syphilis of the nervous system the characteristic reaction in the blood is an antigen reaction, and that it indicates the presence and activity in the body of the syphilitic virus, while in parasyphilis—for example, general paralysis—there is a different reaction. In this case the reaction in the blood is the antibody reaction, and it gives no indication of any active syphilitic process. These facts are obviously of the highest importance. We have not yet had an opportunity of becoming acquainted with the work on which these conclusions are based.

Relation of General Paralysis to Syphilis.

The nature of the relationship of general paralysis to syphilis is a matter which has, since its first inception by Fournier, been productive of much investigation and many theories. The term "parasyphilitic" is but a concise restatement of the fact that there is undoubted and close relationship between the diseases included under that term and syphilis, and does not in any way explain the relationship.

The nature of the complement deviation reaction in general paralysis is especially a matter calling for much further investigation. That a disease the syphilitic

nature of which has only recently been definitely proposed and its relation thereto still generally considered secondary or remote, should give a reaction—the specific nature of which, for syphilis, investigation more and more definitely shows—in a greater proportion of cases than in any stage of the original infection, is a fact both unexpected and undoubtedly difficult to explain on the original conceptions of the nature both of the reaction and of the disease. One or other or both of them seem to require modification. Opinions on the matter are very ill-defined and uncertain. At present it appears to be taken as undoubted that the relationship between the parasyphilitic diseases and the *Spirochaeta pallida* is an extremely remote one, aud due not to the direct action of the spirochaete or its toxins on the tissues concerned, but rather to certain degenerative processes brought about by a profound alteration in the whole metabolic life of the body, the result of an infection with syphilis at some time more or less remote.

The apparent uselessness of mercury and other anti-syphilitic remedies in preventing the onset and progress of these diseases, and their steadily progressive course, their occurrence in cases of mild or even unnoticed syphilis, and where the primary disease has been vigorously and apparently effectually treated, and the ineffectiveness hitherto of the search for the spirochaete, are taken to indicate this.

But may not the theory of latent spirochaete infection, as suggested to explain late tertiary syphilitic manifestations, be applied also to general paralysis? To explain late syphilitic lesions an analogy may be drawn from other infective diseases, such as tubercle, and many protozoal diseases in man and animals. It certainly seems possible that in an apparently cured or recovered case of syphilis, an attenuated infection, or a focus, encapsuled, or otherwise rendered temporarily incapable of producing toxic results, and possibly likewise incapable of producing demonstrable "antibody" formation (meaning thereby the bodies which produce the specific complement deviation of the Wassermann reaction), may exist in the body, capable under special circumstances of being roused into activity, and of giving rise to tissue change, local or general, slight or extensive, with the accompaniment of the formation of deviating bodies. It has been proved, by the observation among others of Furecz,[68] Murero,[67] and Katzenstein,[66] that the *Spirochaeta pallida* may be found in old healed lesions even after two years, and after intense and prolonged treatment. Reuter and Schmorl,[65] and Benda,[64] found the spirochaete in the vessel walls in old syphilitic arteritis, an exceedingly common lesion, by the way, in general paralysis. Wechselmann[63] is definitely of the opinion that in "occult syphilis" the disease may be present in the internal organs. That a latent infection may exist capable after long periods of quiescence of renewed activity and the production of active pathological changes can hardly be doubted.

The disappearance, under treatment and otherwise, of the Wassermann reaction, and the latency and recrudescence of syphilis in its various forms may thereby be explained. Two cases examined by us are of interest in this connexion. In one case in which an ulcerated swelling of the tip of the tongue caused anxiety, a most definite Wassermann reaction was given, the original infection having occurred thirty-two years previously; in the other, a condition of buccal leucoplakia occurred many years after the original infection, which had been actively treated by kataphoresis, etc., and the test proved completely positive. It is to be noted that Fournier classes this condition among the parasyphilitic diseases.

In general paralysis there seems no valid reason for considering it as very improbable that the organism may not have been during years in which the disease was apparently "latent," present in or acting upon the central nervous system. The alternative conception, of an altered condition of the body fluids or blood, or of one of the internal secretions; or, as held by Mott[47] to be most probable, of a "latent toxin," produced by the action of the original infection with spirochaetes, which then presumably died out; which condition remained unaltered through years of metabolic change, to give rise ultimately to the tremendous physical cataclysm the results of which we know as general paralysis, is one of which it is difficult to form a clear conception. It seems to us more easy to account for general paralysis on the supposition, not as far as we are aware negatived by any observations, that the syphilitic infection was not, in cases of syphilis which ultimately became paretic or tabetic, ever destroyed or completely latent. Probably these cases never lost the Wassermann reaction which presumably they gave at first. The fact that apparently slight and well treated cases of syphilis have become paretic or tabetic does not negative this. A progressive deterioration, or possibly simply an increased susceptibility to adverse influences of the cells of the system most disposed (by inheritance, stress, etc.) to degeneration, would thus be the result of direct syphilitic toxaemia.

The presence of the Wassermann reaction in the cerebro-spinal fluid of general paralysis, and its absence in cases of ordinary syphilis, and even, except possibly in a very slight degree,[27 47] in cases of cerebral syphilis, and its constant association with the presence in the fluid of certain constituents, albuminous and otherwise, presumably the result of cerebral degeneration, is a matter of great theoretical as well as practical interest and importance. The investigations of Noguchi,[31] Mott,[47] Nonne,[33] and many others on the albuminous, lipoidal, and other substances peculiar to or characteristically increased in the serum in syphilis and in the cerebro-spinal fluid in general paralysis and tabes, though they have much enlarged our knowledge on the subject, are still very far from giving us an exact explanation of the specific complement binding reaction in these diseases, though investigations on these lines can hardly fail greatly to enlarge our knowledge in the near future.

In connexion with the question of the specific quality of the degeneration products present in the cerebro-spinal fluid in general paralysis, our experiments (vide supra) undertaken with cases of extensive hemiplegia in various stages may be of interest. The fact that the cerebro-spinal fluid in general paralysis possesses no spirillicide properties,[14] while the serum of syphilitics does, is an observation which, if confirmed, is undoubtedly of considerable importance. The observations of Kolle and Schatilloff[40] and others, that the complement binding bodies in the blood in infections are probably antibodies *sui generis* and not possessed of bactericidal or bacteriotropic properties, may receive important confirmation from this observation.

The observations of Levaditi and Yamanouchi and others on the use of certain "chemical" substances, and of extracts of normal organs as antigen, and of Blumenthal, etc.,[61] on the increased lecithin content of the blood, etc., in syphilis, has led to the belief that the reaction is only remotely connected with the *Spirochaeta pallida*.

Progressive impoverishment of the body cells in lipoid substances (most marked in those of parts specially affected, for example, the brain in general paralysis) under the influence of a "latent toxin," or of a certain vicious metabolic tendency, the result of previous spirochaetal influence, is taken to account for the presence in the cerebro-spinal fluid and serum of a substance which acts as a complement binding antibody. The formation of a hypothetical soapy substance round the fat globules in the infected liver is taken to account for the superior antigen effect of an extract of that organ. Thus we are led to theories which are held by many to account for the reaction in general paralysis. According to Mott it is apparently the reaction of the body itself against the spirochaetal infection which (contrary surely to other reactions to chronic infection, unless amyloid disease be an anology) produces changes which result later in a condition whereby the cells of specially vulnerable parts have their vitality so lowered that progressive degeneration inevitably ultimately ensues, and manifests itself, for example, in general paralysis, with the production of the various degeneration products which bring about the Wassermann reaction.

Is, then, the reaction in the cerebro-spinal fluid of general paralysis identical with that in the serum of early syphilis? If so, we must suppose an identical influence to have produced the bodies, whatever their nature may be, which give this specific complement binding reaction. In early syphilis the direct influence of the spirochaete can hardly be questioned. In general paralysis the accepted conception, as we have seen, is that of a progressive "degeneration" of the nervous elements in the cortex, the result of toxic influence acting only indirectly, and quite

independent of an actually present syphilitic toxin formation. The existence of definite tissue changes, and of the production thereby of bodies whose importance in the vital economy, and also in reactions involving complement deviation, is undoubted, cannot, of course, be denied. But, in considering the nature of this deviation reaction, its definitely specific nature in active syphilis must ever be borne in mind; whatever be the chemical composition of the substances concerned, they are definitely and specifically syphilitic in their reaction.

From these considerations we are hardly content to accept unreservedly the view that this specific reaction is only due to some body or bodies quite accidentally and fortuitously produced by the tissues as a remote result of the presence of the spirochaetes. We are persuaded that these bodies—chemical as they must be in their nature—will ultimately be shown to be due in the most direct manner to the action of the infective agent, and to be, therefore, antibodies produced by the spirochaetal antigen.

The view that lipoids and lecithin as such are the only substances playing a rôle in these reactions is also not a little doubtful.

It would seem almost incredible that in syphilis alone the characteristic and specific result of infection should be the production in the body fluids of antibodies (in the complement deviating sense) consisting of substances of almost universal occurrence normally throughout the system, and of vital importance in the ordinary bodily metabolism. It surely seems more likely that in syphilis, as much as in other infections, substances of a definitely specific nature are produced, the presence of which can be determined by the complement deviating reaction, even though their exact origin and rôle may still be uncertain. The complement binding reaction in syphilis is probably just as definitely (and no more so) a "true antigen antibody reaction" as similar reactions in other infections.

A General Consideration of the Nature of the Reaction.

The numerous tests, chemical and biological, which have recently been devised to bring out the remarkable differences which exist between syphilitic and non-syphilitic serums and between the serums of patients at different stages and in various phases of the disease, have resulted in a corresponding activity in the formulation of theories to account for the results obtained. The masterly presentation of the whole subject by Mott[47] is full of interest, bringing to a focus, as it does, the most recent discoveries of the various workers on the subject and the theories evolved to explain them. It seems to us, after a fairly full examination of the literature, that there is a tendency in some quarters to unduly question the directness of the relationship of the *Spirochaeta pallida* to the various phases of syphilis, and especially to the phenomenon of complement binding between syphilitic serum and the "syphilitic virus." The opinions regarding the specificity or otherwise of the substances concerned in the reaction are most difficult to interpret. First the antigen then the antibody is pilloried as being solely some relatively simple chemical body with no pretence to being a "true antigen" or "true antibody." Yet the antibodies at any rate appear to be produced, in the human body at least, in no other way than by an infection with the *Spirochaeta pallida* or possibly of closely allied organisms, and it is certainly found to produce specific complement binding when combined with the syphilitic virus.

The test was undoubtedly founded on the analogy of the Bordet-Gengou reaction, in view of the specific causation of syphilis by infection with the *Spirochaeta pallida*, the only difference being in the use of extract of a highly-infected organ as antigen. Tuberculosis had just previously been investigated by Wassermann and Bruck[7] on similar lines, and the use of extracts of highly-infected organs as antigen instead of pure culture of the causative agent is fully justified, though with limitations and precautions, as pointed out by Meyer.[62]

Working with spirillary infections Kolle and Schatilloff[40] obtained definite complement binding in serums from patients suffering from relapsing fever, using as antigen extracts made from organs and blood rich in spirilla. In our own work one of us (A. G. B.) obtained definite results in serums from cases of plague, using as antigen a watery extract of the spleen of a guinea-pig dead from plague (inoculation), prepared in exactly the same way as the extract of syphilitic liver. It seems generally agreed that the best antigen for syphilis can be prepared from a liver rich in spirochaetes. That there is in syphilis, in some respects at all events, a specific antigen antibody reaction definitely connected with the spirochaete and analogous to similar reactions found in other infections, it is difficult to escape. The fact that various bodies of known chemical composition and extracts of organs other than syphilitic will in some degree give the reaction cannot, we think, be taken to negative this view.

In the first place, it must be remembered that the exact nature of a "true antigen antibody" reaction has still to be defined and the chemical composition of the interacting substances ascertained. Kolle[40] says that "all complement deviating antibodies are antibodies *sui generis*," and not true protective substances, as are bacteriolysins and bacteriotropins.

Interesting observations bearing on the question in relation to syphilis are given by Wechselmann,[63] who gives reference also to others dealing with the question of the relation between the deviating bodies in the blood in syphilis and the protective substances. It is also ably dealt with by McKenzie[35] as well as in the paper by Mott[47] already referred to. The reaction in infection would thus seem to be placed in the same category with the complement binding which occurs with all antigen antibody reactions—for example, in the case of a serum and an antiserum with or without precipitin reaction. It seems necessary under this supposition to consider that certain chemical substances of a highly organized nature—for example, albumens in the body of the infecting organism—produce the antibodies whose presence is evidenced by complement deviation, and that the receptors involved and their immune bodies are not in the case of bacterial infections identical with those which are concerned in immunity production. In any case, in the matter of syphilis it does not seem justifiable to characterize the reaction as not a true antigen antibody reaction, since the exact nature of the reactions which are acknowledged as such are so little understood.

With regard to antigen, the fact that the syphilitic antigen is in some degree, at least, soluble in alcohol is stated by Mott to prove that it cannot be a true antigen. But can the solubility or otherwise in alcohol be taken as the sole test of a "true antigen"? We are not ourselves aware of any sufficient evidence to show that a specific antigen antibody reaction may not be obtained with definite chemical agents in other diseases than syphilis. Possibly all complement binding reactions will eventually be explained in this way. The specific nature of the reaction in cobra venom is undoubted, yet, as Preston Keyes[32] showed, complement may be furnished by a definite chemical substance, lecithin. But all complements are not necessarily lecithin. It is interesting to note that the same substance is a substitute for antigen in the syphilitic reaction. The composition of the body of the *Spirochaeta pallida* has yet to be determined. Mott, indeed, states that the spirochaete consists of a viscid plasm covered with a membrane consisting of lipoid substance; though in spite of this he considers that "the reaction has nothing to do with the *Spirochaeta pallida*." It would almost seem that the minds of many observers have been so diverted by the discovery of the possible explanation of the Wassermann reaction by the interaction of known chemical substances that their attention has in some degree been drawn from the biological aspects of the test. All "antigen antibody" reactions are probably ultimately chemical in their nature, and sooner or later the nature of the substances which act as antigens in infection, whether contained in the substance of a micro-organism or formed by its influence and of the other reacting substances, will be more or less exactly determined. Much more light requires to be thrown on the subject before the exact nature of the Wassermann reaction and its relation to the *Spirochaeta pallida* is understood. Our knowledge of the nature of the reaction of the body to chronic infection in general is still in its infancy. But undoubtedly any conception of the nature of syphilis and explanations of its observed phenomena, to be fruitful in results, must take the fullest cognizance of its character as a chronic infection with the *Spirochaeta pallida*.

Conclusion.

The whole subject of syphilis has reached a stage of great interest and importance. For years the extraordinary clinical manifestations of the disease and the difficulty of obtaining a definite clue to its exact pathology engrossed the attention of observers of the highest powers of observation. Increasing attention was also given to the national importance of the disease as a factor in the production of degenerative processes resulting in serious individual inefficiency and in national physical deterioration. But until the pathology of the disease was placed on a definite and certain basis, all thought in connexion with it was more or less incoherent and action vacillating. To the genius of the lamented F. Schaudinn, in the view of practically every authority on the subject, are we indebted, by the discovery of the *Spirochaeta pallida* and of its rôle in the production of the disease, for an "open sesame" which will undoubtedly result in a coherent and definite conception of syphilis in all its aspects, biological, pathological, and clinical, and will thus clear the way for organized efforts, founded on definite knowledge of the etiology of the disease, for neutralizing its effects, and in some measure at least, minimizing its incidence. The great discovery of Wassermann—a discovery which, whatever the ultimate theoretical explanation of it may be, was undoubtedly based on the conception of syphilis as an infective disease—has opened the way for investigations into which very many workers in the field of biological research have seen sufficient inducement to enter, and new light is almost daily being thrown on the nature of the disease and of its manifestations. The method of complement deviation upon which this most important and far-reaching means for extending our knowledge of the nature of syphilis depends, is one which has proved highly fruitful in results in other fields of investigation, and engrosses a large amount of attention in both its theoretical and practical aspects. The conditions which lead to active or passive anticomplement action and the explanation of the action, the nature of complement, antibody, antigen, etc., and the meaning of their variations under different conditions are being studied with such vigour that our knowledge of them will undoubtedly soon be much more definite and certain. Our conception, indeed, of infective disease in general, and probably of many diseases not at present so regarded, will, in the not remote future, be so altered and enlarged that our present knowledge will seem rudimentary and crude. It is more and more becoming apparent that every man aspiring to practise medicine must in the future, if he wish to keep abreast of the advances, not only in the science, but in the art of medicine and surgery, have a very much fuller and more practical acquaintance than is at present considered necessary with the knowledge now available, as the result of an enormous amount of research and thought, of the mechanism and conditions of infective disease.

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NOTE ON ALCOHOL IN RELATION TO MULTIPLE NEURITIS.

By JUDSON S. BURY, M.D.Lond., F.R.C.P.,
Physician to the Manchester Royal Infirmary.

Nearly nine years have elapsed since the epidemic of multiple neuritis which occurred in Manchester and the surrounding district. It will be remembered that during the latter part of the year 1900 a great increase in the number of cases of multiple neuritis was noticed, and especially in persons who were drinking beer. The idea occurred to many medical men that there must be some unusual toxic substance present in the beer, but it is to my colleague, Dr. Reynolds, that the credit is due for first thinking of arsenic, and for first detecting its presence in the beer. His discovery was soon confirmed by others, and the matter was thoroughly investigated.

Before this epidemic it was assumed that the development of neuritis in an alcoholic subject was due to the action of alcohol on the peripheral nerves. Doubts as to the correctness of this assumption were raised by the discovery of arsenic in the beer, and by the views expressed by Dr. Reynolds. The epidemic drew attention to the importance of considering not only whether neuritis in a drinker might be due to the presence in his beverage of some poison other than alcohol, but also as to whether alcohol by itself could cause neuritis.

In his account of the epidemic Dr. Reynolds stated:

> I have for many years doubted whether ethylic alcohol *per se* does cause peripheral neuritis at all, and I have personally felt more and more confirmed in this opinion each succeeding year.

Again, in responding to the discussion on his paper read at the Medico-Chirurgical Society he said:

> I am willing to admit that alcoholic neuritis exists, but I am not convinced thereof.

He further threw out the suggestion that all cases of so-called alcoholic neuritis might be really due not to alcohol but to arsenic. More recently he has stated that he never meant to deny the existence of alcoholic neuritis, but his belief is that it is of rare occurrence and usually slight in degree.

Such statements were opposed to the generally accepted views on the subject, and it was obvious that subsequent experience was necessary before their correctness could be

estimated. Now, if arsenic were the chief cause of so-called alcoholic neuritis, we should expect to find a great diminution in the number of cases occurring after the epidemic, that is, when the beer was free from arsenic. On the contrary, the percentage of cases admitted to the Manchester Royal Infirmary since the epidemic is almost identical with that of cases admitted before the epidemic. Thus, if we take the seven years included by the years 1892–1898, there were 109 cases of peripheral neuritis out of 9,300 medical in-patients, that is, 1.01 per cent.; whilst in the seven years, 1901–1907, there were 116 cases out of 10,630, that is, 1.09 per cent. But in the year of the epidemic, namely, 1900, there were 62 cases out of 1,343, or 4.6 per cent.

Hence it is clear that, before and after the epidemic, arsenic, if present at all in the alcohol consumed, was in very small quantities. Further, it is to be noted that pigmentation, keratosis, and other skin lesions, so prevalent in the epidemic, were not mentioned in the reports of earlier cases by such acute observers as Dr. Ross and Dr. Dreschfeld. On the other hand, we must admit that the majority of Manchester cases occurred in beer drinkers, and that a small amount of arsenic in the beer may have been a contributory factor.

It is therefore desirable to consider the question of neuritis in drinkers of spirits. In this connexion we may note the valuable statistics of Dr. F. Buzzard. In the ten years included by the years 1891–1900, out of 6,458 cases admitted to the National Hospital there were 108 cases of alcoholic neuritis—that is, 1.6 per cent. Of these 108 cases, 25 were due to excess in spirits only, and only 1 case due to beer only. Dr. Buchan, also, in 19 cases of alcoholic neuritis found that whisky was the chief beverage in 18, and the only one in 6.

These results are in accordance with the records of the earliest observers of peripheral neuritis in alcoholic subjects, as witness the graphic account by Dr. Lettsom of the disease as occurring in those who drank brandy and gin in excess, and that by Dr. James Jackson, who attributed the disease to ardent spirits. They are also borne out by the statements of modern authorities on the subject. Thus Dr. T. Buzzard says his experience has been that neuritis is due especially to spirit drinking. The late Dr. Ross, in an analysis of 90 cases, found that the form of alcoholic beverage was as follows: In 22 spirits, in 3 absinthe and vermouth, in 13 beer and spirits, in 5 beer only, whilst in 47 the kind of alcohol was not given. Moreover, of recent years many cases of peripheral neuritis from spirit drinking have been observed in which beer could be definitely excluded. Dr. Williamson has recorded a case from excessive whisky drinking, no beer having been taken, in which the diagnosis was verified by a microscopical examination of the peripheral nerves. In this case three samples of the same whisky gave no reaction for arsenic with Reinsch's test.

Reverting to the question of beer drinking, I may mention that several cases of *well-marked* and *severe* multiple neuritis, which have come under my care since the epidemic, have been carefully investigated, and arsenic as an etiological factor definitely excluded, no trace of this poison being found in the hair or the urine of the affected patients, nor in samples of the beer that had been taken.

It is open to any one to say that it is not the ethylic alcohol but some accidental impurity that is responsible for the neuritis, but until this toxic agent is discovered, which is certainly not arsenic, it is impossible to resist the conviction that alcohol, whether in the form of beer, wine, or spirits, is a common cause of multiple neuritis.

But this statement does not necessarily imply that multiple neuritis is of frequent occurrence in alcoholic subjects, nor that the disease is a direct result of alcohol. Indeed the proportion of alcoholics who suffer from neurit's is a small one, and in such sufferers other factors may play a contributory part. The action of alcohol may be an indirect one in the production not only of neuritis but of hepatic cirrhosis and of changes in the muscle of the heart. It is a reasonable assumption that the lowering of tissue resistance by alcohol permits the successful attack of toxins, derived from micro-organisms or from the changes associated with chronic inflammation, upon the nerves, liver, heart or other structure.

A similar view is held by Dr. F. Parkes Weber, who suggests that the true causes of multiple neuritis of alcoholic type are infectious, alcohol being merely a predisposing agent.

But whether the neuritis is a direct or an indirect result of the poison there is ample evidence to justify the view that well-marked cases of multiple neuritis (although not common) are produced more frequently by alcohol than by other poisons.

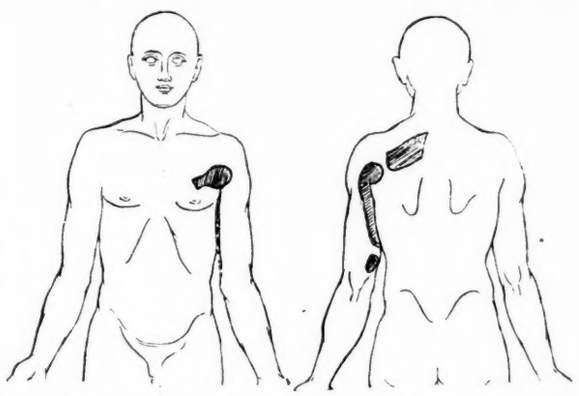
PAROXYSMAL TACHYCARDIA DISAPPEARING AFTER AN ATTACK OF HERPES ZOSTER.

By ALAN C. TURNER, M.R.C.S., L.R.C.P.,
SHEFFIELD.

THE following case presents features of sufficient interest to make it worth recording. The history is briefly as follows:

L. S., aged 15 years, has been under my care for some time suffering from recurrent attacks of tachycardia.

She is rather short, somewhat anaemic, and has never been very strong. She easily takes cold, and has had repeated attacks of influenza. The first attack of tachycardia occurred when she was 5 years old. She awoke suddenly about midnight, feeling "as though she were going to die." She was very pale, and the heart beat very fast. After a short time she vomited a small quantity of liquid; the heart at once became quiet, all symptoms quickly passed away, and she soon fell asleep. This attack was attributed by her friends to a strong cup of tea she had taken the evening before. From that time onwards she had been subject at least every two months to similar attacks. She is obliged to sit still whilst the attack is on, and, however



long it lasts, is unable to take any solid food, but can sip a little liquid. An attack always continues until she vomits, after which all the symptoms quickly disappear. Latterly, she has been given warm salt and water to produce vomiting, and this has cut short the attacks. The vomit consists of one or two tablespoonfuls of sour fluid. The attacks have been attributed at various times to sudden exertion, such as running, to excitement, or to irregularities in diet, but no common etiological factor could be found. In other respects she seems fairly well, the bowels act regularly, and the urine is normal.

The first time I saw her, about five years ago, she was suffering from an attack. She was propped up in bed, very pale, complaining of tightness across the chest and of rapid heart-beat. The pulse was about 160; the impulse was in the nipple line, and no murmurs were audible. Since then I have seen her in several similar attacks; the pulse-rate has varied from 120 to a rate faster than could be counted. They have lasted for various periods, from less than one hour to nearly three days. No drug among the many that have been tried has had any effect in cutting short the attacks, nor has attempted pressure on the vagus been more successful. The attacks have always lasted until she has vomited, when, within fifteen to thirty minutes after the pulse would fall to normal.

On October 10th, 1906, she had severe pain in the left wrist which continued to the next day, when it affected the inner side of the left arm, the front of the left shoulder, and the back. On the morning of October 12th a rash appeared about the left shoulder and arm, the left ear became tender, and she had also some pain in the right wrist. Dr. Arthur Hall saw her at that time in consultation. She had typical herpes zoster affecting the area of the second left dorsal segment—namely, the under and inner part of the left arm, the left breast in the region of the axillary fold, and over the left scapula in the upper part (see diagram).

The eruption ran the usual course, and from the cardiac attack which preceded the eruption by about a month up to the present date—that is, for three years—she has had no recurrence of the tachycardial attacks, her heart is quite normal, and she has never felt in better health than she does now.

There is no family history of any particular tendency to nervous disorders or heart affections.

I have used the term "paroxysmal tachycardia" for this case without thereby meaning to imply any certainty as to the nature of the type of heart beat which characterized it.

Mackenzie (*Diseases of the Heart*, 1908, p. 129) would include under this term only those cases in which a "nodal rhythm" is present. Whether this case fulfilled his requirement or not I cannot say, since no tracing was taken during an attack, and no attacks have occurred since we have been in a position to take such tracings.

The case, however, is typical of the class described by Bouveret (*Revue de Médecine*, 1889) as "La tachycardie essentialle paroxystique," and concerning which Herringham (*Edinburgh Medical Journal*, 1897) has contributed a valuable monograph.

This case in most of its features agrees with one or other of the recorded cases, the variations being chiefly in minor and unessential details. The termination of the original attacks in vomiting and the artificial cutting short of subsequent ones by the administration of emetics is interesting, but does not warrant us in forming any wide inferences therefrom.

The feature which in this case seems to call for publication is the coincidence of an outbreak of herpes zoster of the left second dorsal area with complete cessation of the heart attacks.

Possibly it is no more than a "coincidence," in the popular sense of the word. Cases of paroxysmal tachycardia do cease spontaneously, and it is possible that in this case the cessation may only be a temporary one and that further attacks may occur later. On the other hand, the fact that the second left thoracic nerve is very closely associated with the nerve supply of the heart, and particularly with its accelerator fibres, and the fact that previous to the outbreak of zoster the heart attacks had recurred fairly regularly every two months for ten years, seem to suggest that the two things stand in some direct relation to each other.

TRAUMATIC RUPTURE OF THE SMALL INTESTINE: SUTURE AND APPENDICOSTOMY: RECOVERY.

By FRANK RADCLIFFE, M.D.,
ASSISTANT SURGEON TO THE OLDHAM INFIRMARY.

In the British Medical Journal of May 29th, 1909, Mr. William Sheen of Cardiff records a case of traumatic rupture of the sigmoid colon, in which, after commenting upon the rarity of the lesion, he, by reference to a paper by Berry and Giuseppi, shows that the mortality in the London hospitals in cases of traumatic rupture of the intestine has been over 87 per cent. Under these circumstances it becomes a duty to record such cases, and especially so if, as in the following case, they present features of special interest attached to their treatment.

A. L., aged 33, bricksetter's labourer, was admitted to the Oldham Infirmary on May 10th, 1909, at 6 p.m. He said that he had received a violent blow from the end of a plank, which struck him in the epigastrium; that afterwards he was able to walk a few yards and descend a ladder, but then felt too ill to move. He lay upon the right side with the knees drawn up, and was evidently in great pain. The abdomen was of board-like rigidity, retracted to the scaphoid shape, with the skin thrown into transverse folds, and very sensitive to pressure. The flanks were dull on percussion, but this dullness did not move on changing the position of the patient. The liver dullness was normal in extent. There was no external mark of violence except a few small superficial scratches. Shortly after admission he vomited a little brownish fluid.

It was decided to operate, but the patient would not consent until he had seen his wife, thus delaying the operation until nearly 10 p.m.—that is to say, seven hours after the injury.

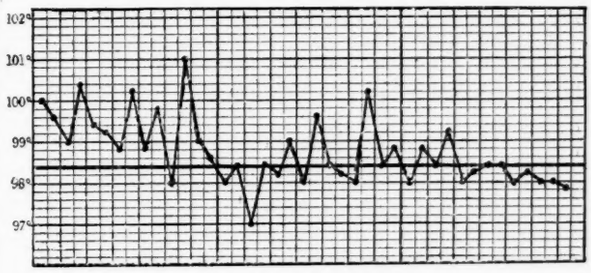
Operation.

Chloroform was administered by the junior house-surgeon, Dr. Gray, and I had the assistance of our senior house-surgeon, Dr. Mackenzie.

An incision was made in the middle line above the umbilicus. On opening the peritoneum there was an escape of brownish, clear fluid. On exposing the liver margin more of this fluid welled up, and with it some bubbles of gas. Examination of the stomach revealed no lesion. The transverse colon and omentum were then drawn up, thus allowing more fluid to escape and with it some flakes of lymph. The intestines were deeply injected, and in places covered with large flakes of lymph. Systematic examination of the small intestine revealed a longitudinal rupture about ¾ in. in length, with pouting mucous membrane on the free border of the jejunum, about 2 ft. from its commencement. The rupture, after sponging with weak mercury biniodide lotion, was closed with a continuous Lembert suture, and the examination was continued but no other lesion was found. A suprapubic incision was then made, and on opening the peritoneum more fluid of the same slightly turbid character and containing small food particles escaped; through this opening a Keith's tube was passed down to the bottom of Douglas's pouch. Another incision was then made through the parietes over the appendix, which was brought through this incision, and its mesentery cut after ligaturing it. The appendix was then amputated, leaving about an inch of stump. This was dilated, and a rubber catheter inserted through it into the caecum and fixed by a purse-string suture; the stump of the appendix was anchored to the skin by two fine silk sutures, and this incision closed round the appendix. The epigastric incision was completely closed by through-and-through sutures and dressings applied. The patient was removed to bed and put into the Fowler position. The whole operation took less than an hour from the first incision to the patient being removed from the table.

After-Treatment.

A cistern was arranged about 2 ft. above the level of the patient's abdomen, and under it a spirit lamp; the cistern was connected by rubber tubing with the rubber catheter, and the tubing clamped so as to allow one drop of normal saline solution with which the cistern was filled to escape every second.



It was found that to ensure the drops of saline having a temperature of 98° F. we had to keep the cistern contents at 150° F. By this arrangement we were able to cause a continuous flow of saline solution into the caecum for forty-eight hours. The tube then came out, and on reintroduction there was so much pain that it was discontinued. The total quantity of saline solution introduced was two gallons.

The patient continually perspired, passed urine freely, and at the end of the time had some incontinence, passing faeces along with a moderate quantity of saline fluid. Moreover, what was more interesting, there was fluid continuously welling up through the Keith's tube, so that the pad over the tube had to be changed every half-hour.

On the fifth day the Keith's tube was removed, as the flow of fluid through it had almost ceased.

On the seventh day some flatus was passed through the appendix.

On the eighth day the stump of the appendix came away as a slough, and left a faecal fistula, which remained open for a fortnight. There was some suppuration along the track of the sutures in the epigastric wound.

The patient was out of bed at the end of three weeks, and walking in the garden within a month.

I relied entirely upon the infusion of saline solution into the caecum to rid the peritoneal cavity of its irritating contents, by causing the membrane to secrete instead of to absorb, and that this occurred was evident by the copious flow from the Keith's tube, continuing for five days, even after the infusion was discontinued.

The man's condition never caused any anxiety after the first twenty-four hours.

If I should meet with a similar case I would diminish the rate of flow of the saline after the first twenty-four hours, for the patient had considerable inconvenience, from the thirty-sixth to the forty-eighth hour after operation, from the rectal incontinence.

It is stated that a motion is about to be submitted to the Duma making vaccination compulsory in Russia. The Inspector-General of the Medical Department has asked the various administrative authorities and institutions to report on the subject, and the Minister of Public Instruction has sent out a circular to the faculties of medicine.

SEVENTY-SEVENTH ANNUAL MEETING

OF THE

British Medical Association.

Held at Belfast on July 23rd, 24th, 26th, 27th, 28th, 29th, and 30th.

PROCEEDINGS OF SECTIONS.

SECTION OF OBSTETRICS AND GYNAECOLOGY.

John Campbell, M.D., F.R.C.S., President.

DISCUSSION ON THE TREATMENT OF ENDOMETRITIS.

OPENING PAPER.

By E. Hastings Tweedy, F.R.C.P.I.,
Master of the Rotunda Hospital, Dublin.

When I was asked to open the discussion on endometritis I accepted the invitation with pleasure and yet with considerable misgiving lest I should fail in my efforts to guide our discussion into a practical channel and find myself involved in the labyrinth of uncertainty which is gathered round the diseases classed as endometritis. In the time allotted for the opening of a discussion it would be impossible to include an adequate description of the pathological varieties of endometritis, so I divide cases of this condition clinically into two classes, acute and chronic; anatomically into corporeal and cervical.

Acute endometritis is a disease which has its origin in microbic invasion. Its symptoms are definite and its course acute; moreover, it is liable to end in the chronic form. Under the title "chronic endometritis" I include all other non-malignant diseases of the endometrium. To those who object to the vagueness of the above classification I urge its merits from a practical standpoint, particularly in respect to treatment. Gynaecologists of the present day combat diseases of the endometrium on well-defined lines. These yield satisfactory results, despite a want of knowledge of the pathological conditions present at the period of their most active treatment. In indicating the treatment which I personally adopt in practice I await with the greatest interest the views of the speakers who will follow me, whether or not they subscribe to my opinions.

Acute Endometritis.

In acute endometritis we have a membrane invaded by pathogenic bacteria, and our treatment should be on the lines which the general surgeon has found applicable in conditions of a similar nature. On general principles, dead areas of infection should be removed, pus pools drained, and the body resistance raised to the utmost to counteract the results of infection.

Surgeons show no anxiety to remove a partially detached slough. They know the exercise of roughness in such a condition will cause bleeding, injury to living cells, and fresh microbic invasion of the tissues. Gentleness, cleanliness, and patience, are the watchwords of the surgeon; these, in my opinion, should denote the principles which guide us in the treatment of acute endometritis. An intra-uterine douche of salt solution, administered with the most rigid aseptic precautions, is the best method of removing pus collections or detached necrotic tissue.

If there is any reason to suspect that there is a disintegrating mass in the uterus, it should be explored with a gloved finger if possible. This operation occasionally necessitates the administration of a general anaesthetic. With the patient in the gynaecological or cross-bed position, the whole or half hand is introduced into the vagina, as this is the only way in which it is possible to pass one or two fingers into the uterus and explore its cavity. Even then it is usually necessary to use the other hand to push the uterus down on to the exploring finger.

Such exploration is only to be advised in acute endometritis of puerperal origin. In non-puerperal inflammations the douche nozzle will probably indicate the presence of pathological tissue such as necrotic fibroids, etc. To drain the cavity of the uterus nothing is better than a wick of iodoform gauze soaked in a 2 per cent. solution of carbolic acid. This gauze should be removed in from twelve to twenty-four hours and reinserted after the next douche. Treatment should be continued as long as septic discharges collect in the uterus. Before the first douche a specimen of the uterine discharge should be taken for bacteriological examination. This affords valuable information from a diagnostic standpoint and enables us to prepare vaccines, which are amongst the most efficient remedies at the disposal of the modern therapeutist.

Acute anteflexions and retroflexions are potent factors in preventing proper drainage. The correction of such malpositions must on no account be neglected. The gauze drain is of especial value in such cases.

Occasionally good results follow the gentle use of the blunt flushing curette. It will have its chief indication in cases where digital examination would be harmful, as for instance, after the recent repair of the perineum. Under these circumstances it often succeeds in entangling and detaching foreign matter laden with deleterious toxins, which would unduly tax the resources of the living tissue if they were allowed to remain. The sharp curette has no place in the treatment of acute endometritis. It is ridiculous to use it with the expectation of removing all bacteria from the uterus. It most certainly injures the living tissue cells. I have known the softened muscle of the uterus to be removed in great masses, the operator believing that he was scraping away necrotic endometrium. With such possibilities present it is obvious that sinuses, veins, and lymphatics can be opened and inoculated, as in vaccination, resulting in further spread of the local infection and possibly in general systemic sepsis. To my certain knowledge such a softened uterus has been ruptured so easily that the surgeon was quite unaware of the appalling accident which had befallen him. Moreover, it is an utterly hopeless undertaking to thoroughly scrape a big cavity, blindly, with a curette. This is comparable to an effort to remove the dust from a table by scraping it with a spoon in a darkened chamber. In my opinion such curetting is no more justified than scraping the male urethra as a means for curing acute inflammation. Until the surgeon demonstrates the efficacy of the curette in acute urethritis I cannot believe that it has any greater field of usefulness in the disease under consideration.

Chronic Endometritis.

The chronic diseases of the endometrium always have some underlying cause, and their cure cannot be complete until this cause is removed. But it is not sufficient to discover this cause when endometritis is established. Pathological tissue has an extraordinary aptitude to imitate in its growth that which has preceded it. When a departure from the normal is established, for a long time it will show little tendency to revert to its former condition. As an example of such imitation, let me cite an ingrowing or cracked toenail. Month after month and year after year the crack will reproduce itself as the nail grows. Surgical help is at length sought. With a knowledge of this underlying fact the surgeon proceeds to cure the defect by avulsion of the nail. Under this treatment the chances are enormously in favour of the new growth reverting to a standard or normal type. Similarly the gynaecologist must remove the lining membrane of the uterus in his serious effort to cure the disease. In the presence of persistent retroversion, heart disease, or other underlying cause, removal of this lining can afford but temporary benefit. On the other hand, I claim that the operation is a complete cure when performed in a proper manner under ideal circumstances; failure should then be expected no oftener than after the avulsion of a diseased nail.

Before going further it is necessary to discuss the symptoms which are usually considered to be associated with chronic disease of the endometrium. The most usual of these will be menstrual in character. Thus, amenorrhoea, dysmenorrhoea, menorrhagia, metrorrhagia, the passage of decidual casts and blood clots, are all well-known accompaniments of the disease. Of referred symptoms, pain in

the back, weakness in the loins, weariness on standing or after slight exercise, headaches, dyspepsia, and constipation have a possible origin in endometritis. However, all these symptoms may be wanting in severe types of endometritis, the presence of which can only certainly be ascertained after miscarriage, abnormalities of the pregnant or parturient state, such as *ante-partum* or *post-partum* haemorrhage, abortions, hydatidiform mole, hydrorrhoea gravidarum, etc. Or, again, the accidental discovery of a large uterus may alone direct attention to the condition. Therefore, whether symptoms are present or not, if dealing with a condition which you have a reasonable suspicion will cause endometritis, treat your patient for endometritis irrespective of precise symptoms. It must be remembered that endometritis is always present before it manifests itself by symptoms.

Curettage.

The operation of curettage requires brief mention. That it is possible to do it in an improper manner is evidenced by the number of cases we observe uncured after what I may term the "bedroom operation." I do not intend to describe the operation in detail, but shall content myself with mentioning a few important points. The most absolute cleanliness is as necessary as for any other surgical operation. The cervix should always be dilated before curetting, care being taken to avoid tearing it. A sharp curette should be used, but not so vigorously that the power of menstruation is ever afterwards abolished. Handling the portion of an instrument that is inserted into the uterus will most certainly introduce germs of a more or less harmful nature. Blood clots and débris should be washed out with saline through a two-way Bozeman catheter. To more completely remove débris, Playfair's sound wrapped with cotton-wool is very useful. The wool should be applied to the sound, both boiled, and then dipped in a 1 to 3 solution of formalin before using. This has the effect of removing little masses which, partially attached to the wall of the uterus, have resisted the douche fluid. Formalin stimulates the uterus to contract. A wick of gauze in the uterus for twenty-four hours ensures drainage, and, keeping the internal os open, stimulates the uterus to contract. The only subsequent treatment is rest in bed for one week.

Rupture of the uterus will happen to every one who has a large experience in curetting. In many instances the rupture is unavoidable, and often requires some skill on the operator's part to recognize it. In the presence of a ruptured uterus curettage should cease; douching also should be avoided. On the other hand, a solution of formalin on a Playfair's sound is very valuable. It can be used gently to swab out the uterus. It will remove detached fragments, cause contraction, and act as a mild antiseptic. Gauze should also be inserted. Under such treatment recovery is absolute and entirely free from untoward symptoms. The endometritis is often cured, and we have seen many cases in which an uneventful pregnancy has followed.

I now enter on an important consideration, namely, the treatment after curettage. I know that some operators do not look upon the primary operation as an ultimate or sufficient cure, and that they resort to frequent douches and local applications before they are finally satisfied with the result, whilst others, discarding the curette, rely entirely upon this mode of treatment. My own practice is opposed to both of the methods. Again, I bear in mind the cure of the ingrowing toenail. I know of no surgeon who, having removed the nail, resorts to frequent applications of strong caustic to the sensitive matrix. Yet, the conditions we have to deal with are very similar in both affections. We remove diseased structures and put the parts in as favourable a condition as possible, trusting that Nature will reproduce normal tissues.

I have heard gynaecologists declare that the glairy or muco-purulent discharge seen at the cervix cannot be cured by the method above suggested. For my part, I never held that it could, nor do I believe that the mucous membrane of the body of the uterus is in any way accountable for this discharge.

It will thus be seen that I give first place by a long way to the curette in the treatment of chronic endometritis. At the same time, in mild cases I have seen much good follow local application of a 1 to 3 solution of formalin in water, without previous curettage, or the careful injection of iodine into the uterus. By this I do not mean that the cavity of the uterus should be swabbed out twice a week, or even once a month. We know that it often takes three months to reproduce a fully developed functionating endometrium. It is at such intervals that I suggest the use of local applications, which should be used immediately after the cessation of a menstrual period.

We need not delay longer on the consideration of drugs which act specifically or indirectly on the uterus and its mucous membrane. Ergot, hydrastine, belladonna, strychnine, salines and iron, are the best known. They certainly have a place in the treatment of endometritis, but by themselves are powerless to cure.

Most of us have followed with interest the recent controversy on the use of the curette. I should like to take this opportunity to range myself with those who believe that no operation for repair of the perineum, the cervix, or for displaced uterus should be undertaken without preliminary curettage. It is impossible to say precisely to what extent symptoms in a given case are due to the lacerated cervix, the prolapsed vaginal walls, the retroverted uterus, or to their accompanying endometritis. I can conceive nothing more calculated to bring discredit upon gynaecology than the neglect of this detail. Thus, when a patient is already under anaesthetic we know that we can curette with safety, we know that endometritis may be present without symptoms which definitely mark its existence, we know that we shall do no harm even if it is not present. Therefore, the practical gynaecologist, disregarding all theoretical objections, will continue to curette, and not sleep any the less soundly because of the fear that occasionally he has curetted without any necessity.

Cervical Endometritis.

Cervical endometritis is a condition that has given rise to much confusion. To it are due the abnormal albuminous and muco-purulent discharges which may cause erosion and are known as leucorrhoea. It must be confessed that the treatment of endocervicitis by astringents applied on a Playfair's probe or through a Bandl's sound yields anything but satisfactory results. On the other hand, great improvement always follows a well-performed partial amputation of the cervix. I use the word "improvement" designedly, for an absolute cure is not forthcoming in the majority of cases. In these cases of incomplete cure I strongly advise dilatation of the cervix and light cauterization of its canal with Paquelin's or the electric cautery, as suggested by Hunner. This application will do more towards curing the condition than weeks of treatment by milder medications. Cauterization can be carried out under local anaesthesia, and when performed carefully seems free from any danger of cervical narrowing.

Finally, there are cases of endometrial disease which manifest themselves by frequent and alarming haemorrhages and defy the usual forms of treatment. For these atmocausis is a valuable remedy. From personal experience I can speak as to its utility, and I have never seen dangerous or untoward symptoms follow its use.

I should like to hear the opinion of those who have practised Bier's treatment for endometritis. The instrument I have used for the purpose is that designed by Dr. Alexander Duke, but my experience has not been sufficiently great to warrant the expression of any opinion.

DISCUSSION.

Professor Fehling (Strassburg) said: I am glad to be the first to offer our best thanks to Dr. Tweedy for his valuable paper. It is well that we should not neglect simpler gynaecological things, like endometritis, in our attention to the great operations. First, I should like to ask Dr. Tweedy whether he recommends the same treatment for acute gonorrhoeal endometritis. I would rather prefer not to do that with regard to the risk of adenitis. Concerning the treatment of acute endometritis in childbed, I regard the inner surface of the uterus like a surgical wound. In cases of beginning phlegmon, the first principle is to leave it alone; even a finger covered with an india-rubber glove may cause fresh infection. For the treatment of chronic endometritis I agree with the indications laid down, especially for such after miscarriage,

placenta praevia, and *post-partum* haemorrhage in the puerperal state. But it is not to be forgotten that in cases of general weakness, chlorosis, and anaemia, local treatment should be deferred. In cases of bleeding there is curetting; in cases of leucorrhoea, local treatment with dilatation, especially of the os internum, then packing with gauze, and no cauterization, only thigenol or scarlet ointment. The student must be carefully taught to avoid perforation. Too violent curettage has often caused bad amenorrhoea; then the patient suffers more than before. I quite agree with Dr. Tweedy that the treatment of cervical leucorrhoea is very unsatisfactory; for this and erosion I generally use the Volkmann sharp spoon. Von Herff has recently recommended for drainage of the puerperal uterus flexible tubes of metal.

Dr. A. Donald (Manchester) said difficulty attached to any discussion on endometritis owing to the various conditions which were classified under that name; some of them were certainly not inflammatory in origin. Dr. Tweedy had not dwelt much on the causes of endometritis, but from one sentence in his opening remarks he seemed to attribute a certain importance to the effect of retroversion and heart disease in producing a relapse after improvement. This is the old "congestion" hypothesis, which supposes that a condition indistinguishable from an endometritis can be produced by passive congestion of the uterus, with the further assumption that a passive congestion of the uterus could be set up by its backward displacement. There was not a vestige of proof in support of either hypothesis. One frequent source of failure after curetting was that the operation was not performed with sufficient thoroughness. It was an operation which, if good results were to be obtained, must be done with most careful precautions and at the same time with a thoroughness that in many cases only a gynaecologist was justified in using. It was an operation that, almost more than any other, required the educated touch. As to a preliminary curetting in plastic operations on the cervix and perineum, it seemed reasonable to believe that a confinement which was so severe as to produce severe lacerations was also probably responsible for some intra-uterine trouble. In any case it could do no harm, and very often it revealed very marked endometritis. The gauze packing after curetting was useful for several purposes—it helped to straighten a flexed uterus, it promoted drainage; but, more important still, it stimulated uterine contractions and favoured that involution which was often one of the main reasons for the operation. Dr. Shaw, of Manchester, had been engaged recently in collecting the results of cases of curetting performed in the speaker's ward in St. Mary's Hospital. The result of these investigations would shortly be published.

Professor Kidd (Dublin) agreed with Dr. Tweedy as to the difficulty of distinguishing between the different forms of endometritis in regard both to nomenclature and treatment. He also agreed as to the risk of curetting the cavity of the uterus in the acute condition, as such treatment might give rise to even more serious conditions. It was very difficult to differentiate between forms of inflammation affecting the cervical canal and the cavity of the uterus. He agreed with those who believed that the diseased condition of the cervix in the great majority of cases was not shut off by the insuperable barrier of the os internum from the cavity of the uterus; therefore, that the common-sense treatment of diseased conditions of the cervix should consist in an extended treatment to the cavity of the uterus. He did not agree with Dr. Tweedy in his contention that amputation of the cervix should be necessary in treating the majority of diseased conditions of the cervix, and thought that by scraping with some such instrument as a Volkmann spoon the diseased condition might be effectually removed and further treatment facilitated. An excessively vigorous application of the sharp curette had sometimes resulted in the removal so completely of the endometrium along with muscular tissue, that the individual operated on had been the subject ever afterwards of a permanent amenorrhoea. Should such a misfortune happen in the case of a young married woman it would be most lamentable.

Dr. Frederick McCann (London) said: I desire to add my tribute of admiration for the able and practical manner in which the subject has been introduced by Dr. Tweedy. He has lightly skipped over any attempt to classify endometritis in detail, and the division of the subject which he made is convenient and practical. I would say at once that curetting is only a part of the treatment of chronic endometritis, for it is a frequent experience in practice to find women who, for this disease, have been repeatedly curetted without benefit. Many of those who suffer from endometritis chronica are in a depraved state of health, neurasthenic, dyspeptic, martyrs to various forms of headache, and whose general nutrition is at a low level. For this reason I have been for some years in the habit of combining the operation of curetting with a six weeks' rest cure. By massage, feeding, and rest, the general condition of the patient is improved; she gains in weight, and a cure may be confidently anticipated. If curetting alone is done, there is little general improvement, and the result is disappointing. Rest in bed for fourteen days is beneficial after curetting, even when it is not thought necessary to advise a "rest cure." As to the technique of the operation, in acute puerperal infections I do not use a sharp curette; indeed, in the majority of cases I do not use a curette at all, and if one is employed it should be as a rake and not as a scraper. I much prefer to sponge out the uterine cavity with squares of gauze on a pair of long forceps, and in this manner the cavity can be more thoroughly cleansed than by the use of an intra-uterine douche. When curetting is done for "chronic endometritis" my rule is to employ two curettes—a blunt curette for the body of the uterus and a sharp curette for the cervix. I prefer to use a blunt curette for the body of the uterus, because I believe that what is intended to come away will do so after the use of a blunt instrument. In some cases the discharge continues after curetting, causing considerable disappointment to the patient, and for such the application, once or twice during convalescence, of a 50 per cent. solution of chloride of zinc to the cervical canal, taking care to wipe away any excess of the fluid, will cure this condition. Lastly, a word as to the so-called gauze drainage. If gauze is used as a drain where pus exists, the gauze becomes sodden and acts as a plug and not as a drain. The introduction of gauze into the uterus may be called for to arrest bleeding after curetting, and then it is used as a plug. Its supposed advantages as a drain in the uterus are largely mythical.

Dr. Thomas Wilson (Birmingham) expressed his satisfaction at the general agreement that appeared to exist in the Section on the advantage of digital evacuation over the use of the curette, blunt or sharp, in cases of acute puerperal endometritis. The exploration of the uterine cavity in these cases was based on the sound surgical principle of removing any foreign body, such as débris of placenta and clots, the presence of which was likely to keep up the inflammation. In the diseases classed for convenience under the name "chronic endometritis," bleeding that did not yield readily to simple treatment formed the indication for curetting; the operation had proved of small benefit when employed for pain or discharge. He did not agree with the suggestion that routine curetting should be done as a preliminary in most operations on the perineum, vagina, or cervix. When preliminary dilatation of the cervix was continued beyond about No. 14 Hegar, a certain degree of lateral longitudinal laceration could generally be observed by passing the little finger up the internal os. Curetting should be considered as merely one factor in the treatment. The speaker had found that the subsequent application twice a month for three months of Churchill's tincture of iodine to the endometrium appeared to diminish the number of relapses following curetting to a considerable extent.

Professor Cullen (Baltimore) said: I have been greatly interested in the able presentation of the subject by Professor Tweedy. The term "endometritis," as it is usually employed, is just as vague as is the term "neurasthenia." Professor Tweedy is to be congratulated on the simple classification of endometritis that he has made. A true inflammation of the endometrium—that is, one in which there has been a pouring out of polymorphonuclear leucocytes or small round cells—has been

relatively rare. In the examination of all the pathological material from the gynaecological department of the Johns Hopkins Hospital during a period of four years we only found 48 cases of endometritis—in short, on an average, only one a month Often when examining the endometrium where pus tubes were present we were surprised to find that the mucosa was perfectly normal. The almost perpendicular arrangement of the uterine canal favours good drainage, thus enabling the uterine mucosa soon to regain its normal character. On the contrary, the fluid in the tubes keeps accumulating; thus the inflammation of the tube long continues. I agree fully with Professor Fehling and Dr. McCann that in many cases the uterine discharge is due to a run-down condition of the patient, and not to any local lesion. In such cases a general building up without any local treatment will bring about the desired relief.

Professor A. Smith (Dublin) divided endometritis into acute, gonorrhoeal, and chronic. He recommended in all acute cases intrauterine exploration if the symptoms were not relieved by douching, but no intrauterine douche should be employed in gonorrhoeal endometritis. Strict rest, frequent hot compresses, and antiseptic vaginal douching gave the best result. He advocated the use of the curette in chronic endometritis, and emphasized the necessity of microscopic examination in ascertaining whether there was any increase of the glandular elements foreshadowing malignancy. In that way patients could be kept under control. Over-dilatation of the cervix was dangerous by causing lacerations where the cervix was small, and he therefore recommended primary dilatation with a laminaria tent. Erosion he considered as an overgrowth of the columnar epithelium displacing the squamous epithelium of the cervix which grew down into the substance of the cervix, and gave rise to an enlarged mucous secreting area. He recommended scarification of the erosion and bathing with sulphate of copper solution, 30 grains to the ounce.

Dr. Henry Jellett (Dublin) said he was glad the Master of the Rotunda had confined himself to the practical points in discussing the subject. The discussion served to show that there was still a great deal of difference of opinion on points which seemed obvious in the treatment of this common condition. This showed that there were more roads to success than one. He had, however, to differ from the opinion that curetting, if properly done, always caused a final cure. The uterine glands dipped below the portions of mucous membrane abraded by the curette, and it was, therefore, impossible to remove all the possible sources of a future infection or prevent recurrence of the disease. He thoroughly agreed that there should be no vaginal operation such as perineorrhaphy or amputation of the cervix, without an accompanying curetting, and would like to supplement that statement by saying that there should be no curetting without any adjunctory vaginal operations which might be necessary; if there was a retroversion or an erosion a suitable operation should be performed, and they should not trust to the effects of the curetting to remove the effects of such complications. There were some who, like Dr. Donald, thought that the displacement was the result of the endometritis, and others who thought endometritis was the result of the displacement; the speaker would like to say "a plague on both your houses!" cure both conditions and you will cure the patient. In acute endometritis he strongly recommended the use of 50 per cent. solution of formalin injected into the uterus and quickly washed out. It was not so dangerous as chloride of zinc.

Dr. Frederick Edge (Wolverhampton) said: Curetting is not always good practice before plastic operations, and is only necessary when the condition of the uterus is doubtful and there is discharge. It is impossible to get all diseased endometrium away by the sharp spoon, and consequently we only remove soft, diseased parts. The use of strong caustics after curetting is illogical, as they destroy the uterine wall, and if they are combined with gauze plugging this retains the caustic fluid and lengthens and intensifies the amount of sloughing. The uterine cavity should be dilated with hollow dilators, as otherwise the dilator acts as a ram and drives fluid from the uterus into the tubes, and possibly into the peritoneal cavity. Hence the peritonitis noticed sometimes after dilatation. For catarrhal endometritis with neurasthenia, with general abdominal relaxation and Glenard's disease, where so often nephropexy and six weeks' rest is ordered, the gynaecologist will often find that curetting, with the rest, will obviate the necessity of nephropexy, and it has none of the crippling results which we have all seen after nephropexy. Curetting cures haemorrhagic endometritis. Dilatation and discission cure dysmenorrhoeic endometritis. Septic and catarrhal endometritis require dilatation, curetting, drainage, douches, aperients, rest, and tonics.

Dr. S. J. Cameron (Glasgow) thought that all acute cases associated with infection of retained products of conception should be treated by dilatation and digital removal of the remains of the ovum. In acute cases of corporeal endometritis due to gonorrhoeal infection no curettage should be performed, but when the disease reached a subacute stage the cervix should be dilated, but curettage should be avoided. Instead, the interior of the uterus should be swabbed out with some antiseptic. They were too apt to look upon endometritis as being always an inflammatory process, whereas it was in very many cases a new growth, and, as such, a cure was often possible. In all chronic cases of endometritis of the uterine body a sharp curette should be used. The flushing curette should be avoided, especially by those who were not constantly using the instrument, as the uterus might be unconsciously perforated, whereupon the peritoneal cavity might be filled with an antiseptic solution. It seemed to him that, although cervical endometritis had usually a common origin with corporeal endometritis, yet the cervical erosion was often more difficult to cure than the endometritis of the body. Probably a cervical erosion was the most difficult of all endometrial lesions to cure, if it were of gonorrhoeal origin. In these cases he believed that the erosion should be scraped and then cauterized. If a cure was not effected, he did not hesitate to excise this portion of the cervix. He was of opinion that a preliminary curettage should be done wherever the operation of perineorrhaphy had to be performed. Rest and tonic treatment often gave admirable results after a curettage for chronic endometritis. He would like Dr. Tweedy to inform him whether he had met with cases of endometritis similar to those described by the speaker in a past number of the *Lancet*, in which the endometritis followed ectopic pregnancy. He thought that this was a commoner cause of endometritis than was generally supposed, and believed the endometritis to be then due to infection of portions of the decidual cast which had not been expelled from the uterus.

Dr. A. W. Russell (Glasgow) said an almost bewildering wealth of suggestions had been put forward, but greater stress, it seemed to him, should be laid on the importance of general constitutional treatment, especially the so-called rest cure, which included sexual as well as ordinary physical rest. Sufficient notice had not been taken of the slower form of dilatation by means of tents under the precautions that were now possible. In spite of the many opinions to the contrary, he was compelled, as a result of his own experience, to speak favourably of the judicious use of the proper form of curette—namely, the broad and not too sharp loop curette, which he believed to be in the skilled hand a scientific prolongation of the imperfect finger. With regard to the extent of the operation that might be necessary, he always advised and carried out at one time whatever combination of operations was necessary for the relief of the condition, whether plastic, suspensory, or both together, in addition to the simple curettage of the uterus.

Dr. Munro Kerr (Glasgow) referred especially to the fact that in many operations for endometritis the tissues removed showed no distinct microscopic changes. Many years previously, in Professor Olshausen's laboratory, he had investigated this subject, and had had exactly the same experience as Professor Cullen—namely, that in many cases there were no obvious changes in the mucous membrane. The second matter Dr. Kerr referred to was the fact that in very many cases, in spite of curettage and replacement of the uterus, and even in spite of repeated

curettage, the symptoms of leucorrhoea and menorrhagia continued. There were a considerable number of such cases. They were cases of metritis, and their treatment was most disappointing.

Dr. E. J. MACLEAN (Cardiff) said that the difficulties and risks of rupture of the uterus usually supervened after the passing of dilator No. 9. A distinction should be drawn between curettage and exploration of the uterus. Where, for the latter operation, the major degree of dilatation was required, the amount of unavoidable rupture was sometimes disconcerting. The flushing curette was much preferable, and strong mercurial solutions were to be avoided. As to the particular form of dilators, there was no need to insist on the use of hollow instruments. If solid, having regard to the fact that the uterine cavity was potential only, there was no risk of forcing air or fluid into the peritoneal cavity via the tubes. Where, after the operation, the uterine cavity was packed, the recovery was afebrile in a smaller proportion of cases. It was to be hoped that, as a result of the discussion, the valuable operation of curettage for certain cases of endometritis would be preserved in an amended and approved form.

The PRESIDENT (Dr. John Campbell, Belfast) regarded a sharp curette as essential. The diseased tissue of the endometrium could not be brought away with a blunt instrument.

AN OVARIAN TUMOUR, CLINICALLY MALIGNANT, ARISING FROM THE OVERGROWTH OF LUTEIN CELLS.

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ON account of the peculiar histological characters of the tumour I am about to describe, of its producing marked symptoms of malignancy out of proportion to what the microscopical appearances suggest, and of the rarity of the case, I have ventured to bring this communication before the Association.

Solid tumours of the ovary are usually an interesting study because of their comparative rarity, varying degrees of malignancy, and their obscure mode of origin. Apart from fibroma, fibromyoma, sarcoma and carcinoma, no mention, so far as I have been able to find, is made in the literature of gynaecology of the etiological connexion between lutein tissue and ovarian tumours, except in an abstract by Dr. Fothergill of a paper by Grouzdew, published in the *Archives of Gynaecology*, 1903. In this paper the following cases are mentioned: (1) Voigt has described a case of Porro's operation in a twin pregnancy, performed on account of a malignant ovarian tumour. This was as large as a man's head, solid, and of soft consistence. The growth was yellowish-grey in colour, and was said to represent an enlarged corpus luteum. (2) Scholler and Pforringer report a case of lutein retention cysts in a case of hydatid mole in which lutein cells had shown an energetic proliferation which they described as a sort of "carcinomatous degeneration"; and (3) in the same article Grouzdew relates a third case in which both ovaries showed small irregular growths, the peritoneum was studded, the lymphatic glands enlarged, and there was ascites. The ovaries contained a proliferation of lutein cells. He suggests the name, sarcoma ovarii cysticum luteino-cellulare.

A very similar case to my own occurred under the care of Sir W. J. Sinclair in November, 1899, and a short clinical account, together with a microscopical section of the tumour, has been kindly sent to me by Dr. Fothergill. The patient, aged 30, was in hospital a long time, and probably was very ill on admission, and in consequence the operation was delayed. There was ascites, and the tumour on either side was the size of a fetal head; it was rounded, and showed bosses on the surface. The patient died about two months after the operation. There is no information to hand about recurrence, but the circumstances of the case pointed to malignancy of the tumour. Microscopically the appearances are the same as in my case.

The tumour upon which this communication is based was removed at the Redditch Hospital on October 5th last year.

History.—The patient was referred to me by Dr. Mogg of Feckenham. Her age was 38, she had been a widow ten years, had four children, of whom the youngest was 12 years old. Menstruation had been regular up to three months before, after which there had been a slight show of one day's duration every fortnight. She had complained of pain of a sharp character in the abdomen causing nausea and vomiting for seven weeks, and had lost strength and flesh, and had sweatings.

State on Examination.—The lower part of the abdomen was enlarged by a hard, movable lump the size of a fetal head. There were signs of free fluid in the peritoneal cavity. On opening the abdomen, solid tumours of both ovaries were removed and there were two pints of a clear yellow ascitic fluid.

Result.—The patient made a very slow recovery owing to extreme weakness. After going home on November 16th, she never really regained her strength, and she died of exhaustion on March 22nd, or five and a half months after the operation. No *post-mortem* examination was made.

Pathological Report.—The right ovary of greyish-white colour is solid but of soft consistence except for one small cyst near the surface. The size of the tumour is about that of a fetal head, 5 in. by 4½ in. by 3 in.; its contour is uneven by irregular bosses projecting on the surface. On section two parts may be recognized, a large central patch of opaque, pale yellow, firm, and compact tissue, and a surrounding clear, grey-coloured, oedematous-looking tissue of which the projecting bosses are composed, and which on squeezing exudes a watery fluid as from a sponge. Small irregular yellow patches are seen scattered in this loose tissue. The left ovary, enlarged to about the size of a green walnut, 2¼ in. by 1¼ in. by 1 in., shows on section the same appearances. In the cortex there are seen to be many small follicles.

Microscopic Examination.—The features of the tumour under the microscope are the growth of new cells which undergo degeneration, the formation of a fibrous connective tissue network the result of a reactive change, and an oedematous infiltration of the ovarian stroma and connective tissue. The cells may be described as follows: The most characteristic are some "stuffed cells," which I have so called because many of them have the appearance of being stuffed with protoplasm. Their distribution is very variable; in places they appear in large patches, being wedged or closely packed together, while in others small groups only are to be made out, and here and there they form columns of cells, and between these lymphatic channels are seen. These cells are large and round, oval, or polygonal in shape, and contain coarsely granular protoplasm, which stains faintly. The nucleus is small and flattened against the cell wall by an excess of protoplasm. In some cells the nucleus is enshrouded or hidden. The tissue of which these cells form a part corresponds to the opaque yellow-coloured compact patches noted in the gross section.

In many parts there are also masses of cells of the same shape and size as these stuffed cells. They contain a relatively small nucleus, which in most of these cells is similarly displaced against the cell wall, but the protoplasm, instead of being granular, has become vesicular and translucent and does not stain. In some cells part only of the protoplasm has become vesicular while the remainder is still granular. This alteration in appearance is, I think, due to a degenerative oedematous infiltration of these cells. Staining with osmic acid does not show the presence of fat. In other parts the ovarian stroma cells appear somewhat swollen as if they are about to become converted into tumour cells.

In the less compact and loose outer zone the tumour is largely made up of a fine and a coarse reticulum of connective tissue which stains readily with van Giessen; the meshes of this tissue contain but few cells and appear wide as if stretched by an oedematous infiltration. The connective tissue trabeculae represent ovarian stroma in part and also to some extent the result of a polynuclear round-celled infiltration which is brought about by the irritation of the new growth. The final stage of the degenerated vesicular condition of the stuffed cells is in certain situations shown by patches of necrosis produced by pressure from blood extravasation and oedematous infiltration of the tumour.

NATURE OF THE GROWTH.

It is in attempting to classify this tumour that difficulty arises, and it is my object in this short paper to try to consider:

1. The nature of the tumour in reference to other recognized growths of the ovary, as fibroma, sarcoma, and carcinoma.

2. The origin, if it is possible to determine, from any pre-existing and normal tissue in the ovary—namely, lutein tissue; and

3. The question of malignancy.

1. Fibroma of the ovary is often bilateral, and with it ascites commonly exists, but it is a slow-growing tumour, and does not produce any constitutional symptoms. On section a fibroma has a whorled appearance, and microscopically it is represented by spindle cells like those of

the ovarian stroma. There is nothing, I think, in the tumour like the small round-celled sarcoma; nor, again, is there any alveolar or tubular arrangement really suggestive of carcinoma. In places where there are columns of stuffed cells between lymphatic channels there is a gross resemblance to endothelioma, but the cells are not like sarcoma cells, and there is not the same degree of proliferated lymph spaces.

2. I am indebted to Mr. B. J. Ward, to whom I showed sections of the tumour, for the suggestion that the cells resemble in very many particulars those of the corpus luteum. How far this is the case, and the further consideration as to whether lutein tissue with its subsequent degenerations may give rise to a tumour, I shall now consider.

It is generally recognized that the growth of lutein cells takes place by a differentiation of the ovarian stroma immediately surrounding the developing ovum in a zone called the theca interna. Sections taken through a well-developed corpus luteum show a festooned yellow ring which is made up of polygonal epithelioid cells containing granular or vesicular protoplasm and small round nuclei. The lutein layer is traversed by numerous tolerably thick connective tissue partitions which are richly supplied with blood vessels and lymphatics. Lymphatic spaces are seen between the lutein cells.

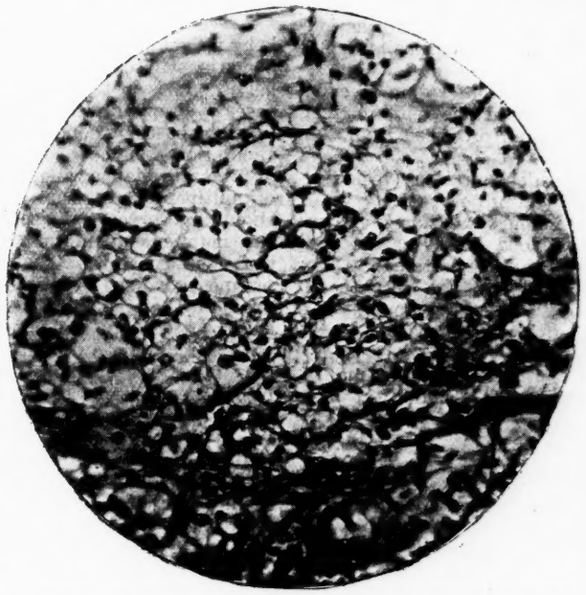

Fig. 1.—Area of vesicular cells, in which the cell protoplasm is translucent. The nuclei in many cells are pushed over to the cell wall. This probably represents a stage of degeneration.

In the tumour the appearance of the yellow patches seen in the gross section is not unlike lutein tissue, and microscopically the stuffed cells have characters very similar to lutein cells; as compared with the latter they tend to be round or oval rather than polygonal in shape, and the nucleus to be pushed to the periphery. The protoplasm is coarsely granular in both, but while the lutein cell is swollen the tumour cell seems stuffed with it. The nucleus in both is small. When we come to consider the subsequent degenerated forms of cells in both tissues they seem very similar; there is the same clear or vesicular appearance in part or in the whole cell, although this change is more marked in the tumour.

Fraenkel by experiments on rabbits has demonstrated and proved the influence not only of the ovaries but of the corpora lutea on the implantation and the further development of the ovum in the uterus. These results were obtained by destroying with the galvano-cautery all the corpora lutea in the ovaries where pregnancy existed, and in each case the latter was brought to an end. Further, in non-pregnant animals, removal of the corpus luteum brought about atrophy of the uterus; and in the human subject burning out the corpora lutea modified menstruation by deferring it until the following ovulation in 8 out of 9 cases. And further it is well known that in a considerable number of cases of chorion-epithelioma—a disease due to an increased activity of the chorionic epithelium—there is found an excess of lutein tissue taking the form of lutein cysts in both ovaries.

Beyond these established facts concerning the influence of lutein tissue on the embedding and the development of the ovum in the uterus and on the occurrence of menstruation, it is difficult without more evidence to say what part, if any, lutein tissue plays as an "internal secretion" in the nutrition and economy of the body, nor is one justified from this single case to connect as cause and effect the grave constitutional condition with the excess in growth of the lutein-like cells.

Fig. 2.—Section of fully-developed lutein layer of a normal corpus luteum, showing very similar cells to those seen in sections of the tumour.

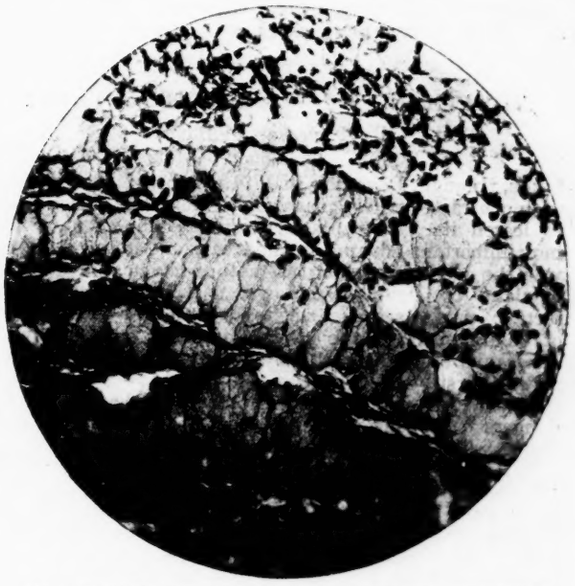

Fig. 3.—"Stuffed cells" arranged in columns and separated by lymphatic channels.

3. Question of malignancy. That the tumour is clinically malignant I cannot doubt. The marked wasting with the occurrence of vomiting before the operation, together with the extreme exhaustion ending in death five and a half months afterwards, points in this

direction. The same kind of weakness was probably present in Sir W. J. Sinclair's case.

CONCLUSIONS.

1. That the tumour was of rapid growth, producing ascites and symptoms of a malignant nature.
2. That pathologically the type of new cell growth does not conform to fibroma, sarcoma, or carcinoma.
3. That the gross sections show a new tissue formation of an opaque yellow colour, not unlike corpus luteum tissue.
4. That this tumour is caused by a proliferation of cells (stuffed cells) which has a close resemblance to lutein cells at the height of their development and produced from the ovarian stroma.
5. That these cells undergo a degeneration (vesicular change) similar to their physiological prototype.
6. That the final stage in the tumour formation is shown by necrosis due to pressure from blood extravasation and oedematous infiltration of the cells and the stroma.
7. That in parts little affected by the lutein cell proliferation there is an extraordinary amount of oedematous infiltration of the ovarian stroma. This is seen in the outer part of the tumour.

I am indebted to Professor Leith for allowing me to have my microscopical sections photographed at the University, Birmingham.

DISCUSSION.

Dr. LOUISE MCILROY was much interested in Dr. Savage's paper. She had experience of a similar case within the last few months, the whole abdomen being filled with an enormous solid tumour of the ovary. This she removed, but the patient only lived for about ten days, not being able to take any nourishment. Dr. McIlroy had the advantage over Dr. Savage of following the case to the *post-mortem* examination, and thus proving the primary growth of the tumour in the ovary. This tumour was not like a sarcoma or an epithelioma, and she suggested to Dr. Savage that in both cases the tumours belonged to the class of the peritheliomata.

The paper was also discussed by Professor FEHLING, and Drs. MUNRO KERR, EDGE, and H. T. HICKS, the chief point raised being the connexion, if any, of similar growths with antecedent vesicular mole, with which they had been found associated in many cases.

TREATMENT OF CONTRACTED PELVIS.

By Professor FEHLING,
Strassburg.

SINCE, through Deventer in 1701, we became acquainted with contracted pelvis, many endeavours have been made to lessen its disadvantages during confinement. The most important advance in the treatment of the condition was the idea of inducing premature labour, spread from England by Denman more than a century ago. No further progress was registered until the introduction of enlargement of the pelvis by Gigli, who, following Morisani's efforts, kept alive symphysiotomy, as brought forward first by Sigault.

To-day we can protect a woman in labour from the dangers of a contracted pelvis much more surely than before. Three different methods are open to us:

1. Knowing the approximate size of the pelvis, we can try to obtain a corresponding size of the child's head. This can be effected by feeding the mother during pregnancy in a sparing manner. On the basis of the so-called Schweninger diet, Prochownik put forward a special form of diet for the pregnant, with which he obtained satisfactory results. Scientifically his proposition was supported by certain experiments made by an earlier assistant of mine—Dr. Reeb. They were undertaken on the same mother animals, fertilized every time by the same male, the one time fed with good, rich food, the second time with bad. The total weight of the young gave on the average in cases of underfeeding 41.2 per cent. less than in cases of good feeding; the total fat is in cases of bad feeding 61.9 per cent. less than when the food is good; the nitrogen and ash content, on the other hand, are not influenced. These results are in keeping with Prochownik's views; but they cannot, of course, be taken as a complete guide to human treatment without further corroboration. The putting on of fat takes place mostly in the last six to eight weeks of pregnancy, and results will be achieved by the withdrawal of food in these last weeks; but it must not be forgotten that initial differences can arise—as when a girl follows a boy, or the reverse.

A second way whereby a small fetus can be obtained is by inducing premature labour at a time when the child's head is still capable of passing through the narrowed pelvis. It is a treatment specially to be considered in cases of simple flattened and rachitic flattened pelvis, and in slight cases of generally contracted pelvis. After much vacillation, it is now generally accepted that the induction of premature labour should be undertaken only in the case of multiparae. In primiparae with contracted pelves, one should first observe the course of a spontaneous birth, but the main reason for not artificially producing labour, is that then the slow extension of the soft parts makes the prognosis worse. Moreover, the time during which the procedure can be adopted is limited. I choose from the thirty-fifth to the thirty-seventh week, induce the labour with a hydrostatic dilator, and seek to obtain, or retain, a cephalic presentation. By this method the results are by no means bad. In the Strassburg clinic 80 per cent. of the children were born alive, and 76.46 of those born living discharged living.

In contradiction to the teaching of Zweifel, I accentuate the fact, with v. Herff and Pfannenstiel (to whose brilliant results one may well refer), that the number of living children at the end of the first year is a really large one (82 per cent. of those discharged alive), larger than in the case of children born by Caesarean section.

From what I have said, I hope this treatment may be recognized as a means which a practical accoucheur can practise with success.

2. Now there are cases to which the foregoing methods are not applicable, or in whom the patient with narrowed pelvis comes under notice for the first time when labour has commenced. For a few of these cases the widening of the pelvis produced by the use of the Walcher position during labour is successful. In the Walcher position, the anterior half of the pelvic ring sinks, and thereby increases the length of the conjugata vera by from 1 to 1.5 cm. It is unfortunately of use only in few cases, but we can employ it with success for the passage of the after-coming head. Its use is of less value in cases of delayed entry of the head into a narrowed pelvic brim.

In hospitals the best means of all is pubiotomy; how far it can be undertaken in private practice has yet to be proved. Let us first decide what enlargement of the pelvis pubiotomy offers. With a space of from 4 to 6 cm. between the pelvic bones at the symphysis, the available conjugata vera is increased by from 1½ to 2 cm.—that is, it measures approximately the same as the conjugata diagonalis, but only when there is free movement at the sacro-iliac synchondrosis. Therefore in cases of oblique distortion of the pelvis one avoids pubiotomy.

The non-rachitic and rachitic flattened pelves come again here into consideration, because in their case the conjugata vera never sinks below 7 cm. In cases of generally contracted pelvis, both symmetrical and asymmetrical, as also in cases of rachitic generally contracted pelvis, the true conjugate should never measure less than 7 cm. In the rarer forms of contracted pelvis (as the obliquely distorted, the triradiate, the osteomalacic, etc.) pubiotomy is to be entirely avoided.

In any given case I do not consider it very difficult to decide upon the indication and moment for operating. The operation is indicated when from the beginning of the labour it is known that there is a disproportion between the size of the child and the pelvis, and when during the course of the labour it is seen that danger for the mother or child is arising therefrom, such as commencing stretching of the cervix, fever, slowing of the fetal heart sounds, and the impossibility of bringing the labour to an end by more usual means. A man in private practice will have but seldom the opportunity of performing pubiotomy. The better course for him, when longer waiting is too dangerous, is to save the mother by a high forceps operation, and, if this fails, to perforate, even a living child.

In hospital work, on the other hand, one is, thanks to pubiotomy, in a position to avoid the perforation of a living child. Pinard put that clearly before us. The one important point is that the child be in good condition. If it is endangered—as, for instance, by prolapse of the umbilical cord—I prefer Caesarean section, because, through it, the child can be more quickly brought into the world. I do not, as a matter of principle, exclude primiparae. As regards the technique, the large skin incisions recommended by Gigli and van de Velde have been given up, and one uses in the open method only the small incision on the horizontal pubic ramus advised by Döderlein. One works then with a blunt instrument from above downwards behind the pubic bone, guides the saw afterwards in the track made, and further round the under edge of the ascending pubic ramus towards the labium majus.

This was my procedure in the first 17 cases; in the 15 later ones I have followed Bumm's subcutaneous method, which is as follows: The needle recommended by him for the purpose is made to pierce the skin, under the control of one hand in the vagina, at the lower edge of the pubis, is then pushed upwards, when possible, under the periosteum, until the skin above the pubis and somewhat to the outer side of the tubercle is reached. A small incision is then made down on to the needle point, the point is pushed through this, the saw attached to it and drawn back in the same course. If, up to this time, severe bleeding has been avoided, it is not likely to occur from the sawing.

As soon as the bone gapes enough—this can be best controlled from the vagina—the saw is withdrawn and at once the subcutaneous wound compressed from outside and from the vagina with gauze, to prevent further venous bleeding and the production of haematomata. Whereas in my first 10 cases I delivered the patient immediately after having sawn through the bone, I now follow Zweifel's advice and await, if possible, a spontaneous birth. The advantage of this is the avoidance of injury to the soft parts, which threatens more especially in primiparae; but if, in the meantime, the child shows signs of danger, the forceps must be used or version undertaken. Immediately after the birth of the child, as also after that of the placenta, the tamponade must be repeated. This remains in place for twenty-four to thirty-six hours; a catheter is introduced for forty-eight hours.

Of my 32 cases, I lost one mother. The placenta had to be manually removed. She died after more than two months from an uncommon form of *coli* sepsis. Of the children, 27 were born alive. This gives a maternal mortality of 3.2 per cent., an infantile of 9.6 per cent. Thus the maternal mortality is lower than that due to Caesarean section, the infantile higher.

The operation, therefore, makes the perforation of a living child unnecessary in a hospital, and comes into competition during labour with relative Caesarean section; during pregnancy, with the induction of premature labour. With regard to symphysiotomy, I have much less experience, but have gained the impression that it gives rise to severer bleeding, as either the corpus or crura clitoridis is torn, and, further, the urethra and bladder are more endangered.

The latest that one hears with regard to pubiotomy shows that also in midwifery fashion plays its rôle. At the present time the newer methods of performing Caesarean section have pushed pubiotomy into the background, but I am convinced that, in spite of this, it will always retain its place in midwifery.

I have, moreover, no anxiety in still performing pubiotomy when, in a case of induced premature labour, the head proves too large to pass through the pelvis.

The introduction of pubiotomy has very much simplified the conduction of labour in contracted pelves. In the first place wait as long as possible—in more than 80 per cent. of the cases the result is a spontaneous birth. Should danger for mother or child ensue, then in practice one must first try forceps, and, if this fails, follow it by perforation. But in hospital, rather than this, so long as the child is in good condition, we employ pubiotomy.

3. The third method of evading the dangers of labour with narrowed pelvis is the radical. One delivers by Caesarean section. The absolute indication is here, when the child lives, very simple—when the true conjugate measures 6.5 cm. or less and the child is viable.

The boundaries within which I perform relative Caesarean section are narrow on account of pubiotomy. The indication is given when the true conjugate of a multipara measures between 6.5 and 8 cm., and when other methods, as forceps, premature labour, even pubiotomy itself, have given bad results for the child. The upper limits (7.5 to 9 cm.) generally accepted as indication for relative Caesarean section I reserve for pubiotomy.

Relative Caesarean section is out of the question for primiparae, especially unmarried ones, for infected cases, and when the child is dead. There is no doubt but that Caesarean section, even in the hands of the best operators, gives a higher mortality than myotomy. In the first place, this arises from the uncertainty concerning the bacterial content of the internal genitals—one cannot ascertain from the look of a patient, who comes into hospital after having been examined outside, whether her genital canal contains only saprophytes or haemolytic streptococci.

In the second place, the conditions under which the wound heals are absolutely different. The wound in the uterine muscle is continuously retracting, especially in the first twenty-four to forty-eight hours. Owing to this the sutures become loose and bacteria can make their way from the uterine cavity into the wound. This occurrence is further aided by the changing states of contraction and relaxation of the organ, whereby the germs are to a certain extent sucked into the lymph vessels. Therein lies, at least according to my opinion, the difference between the healing of the Caesarean section wound and that of other abdominal operations.

Others believe that when the uterine contents are infected the peritoneum is able to stand very much less than the connective tissue of the pelvis; hence the newer methods of Frank (suprapubic Caesarean section), Sellheim (extraperitoneal Caesarean section), with numerous modifications by Veit, Pfannenstiel, Latzko, etc. In this operation it is necessary to push upwards the peritoneal fold lying between the posterior surface of the bladder and the lower uterine segment in order to reach the cervical segment. Numerous operators have shown that it is not always possible to attain this without injury to the peritoneum, but a small injury, immediately afterwards closed, does not seem to matter much. On the other hand, the operations have shown that when a patient is infected with really virulent germs, she is, even by this means, not to be saved. She may die later with or without peritonitis, and this seems only likely.

I do not agree with the point of view of those operators who consider the peritoneum especially sensitive to infection. As we have seen from numerous operations on purulent adnexa, and also from Tavel's varied experiments, the healthy peritoneum can fully deal with bacteria, even streptococci, which have found their way into it. One must only avoid leaving in out-of-the-way spaces stagnating fluid and large surfaces robbed of their serous covering. This can be avoided in Caesarean section, and then the peritoneum is not any more endangered in the uncertain cases than the parametrium, rich as it is in blood and lymph vessels. On the contrary, the latter operation has an advantage in that the incision is made through the thinner part of the uterus, where the blood supply is much less than the cervix. Then, whether one makes a longitudinal or horizontal cut (I prefer the former), one is surprised to see how little it bleeds; the healing of the wound is not interfered with as above, when the thicker part of the uterus is cut through; the wound can readily be stitched in stages; last of all the easily movable peritoneum. Of later births in these cases I have as yet no further experience.

As a matter of fact, the occurrence of rupture of the uterus should have hinted to us long ago that the incision for delivering the child had better be made in the stretched cervix rather than in the thick uterine wall. When the cervical method is undertaken the operation must not be made at the beginning of the labour, but later, when the cervix and lower uterine segment are well stretched, so that one can comfortably cut down on them from above the symphysis. The child is then delivered either spontaneously, with the forceps, or by version this varies according to the case.

I intend in the future to replace the classical Caesarean section by the cervical, expecting better results from it

rather than from the extraperitoneal method. Time and results will teach us what is right.

As regards relative Caesarean section, we cannot give in millimetres exactly between which limits this should be undertaken, since the size of the child's head and its consistence, the force of the pains, etc., also come into consideration. Only by careful observation of the individual cases can this be decided, but then the doctor in a well-arranged hospital will find himself, through pubiotomy and aseptic Caesarean section, in a position to save both mother and child, where earlier one or both had been lost.

DISCUSSION.

Professor MURDOCH CAMERON (Glasgow) said: Professor Fehling has laid more stress on induction of labour and Caesarean section than on the question of the use of the forceps. He has, I think, stated that the chance of the survival of the child is greater after induction of labour than after Caesarean section. Might I ask him how he explains that a child born at the thirty-fifth or thirty-seventh week has a better chance of life than a child born at full term? As regards the method of diet, our experience is amongst poor ill-fed women. I agree with him as to the measurements and operations advocated by him.

Dr. HASTINGS TWEEDY said: Professor Fehling's experience has been so great and his results so good that we in this country are bound by his findings until our own experience is sufficient to enable us to judge for ourselves. Caesarean section is not a rival operation to pubiotomy, but should be considered supplementary. We are now in possession of methods that should make it unjustifiable in hospital practice to perforate a living child. In general practice premature labour will be always preferred to any cutting operation. Accurate measuring of the pelvis is much neglected in our country. Skutch's pelvimeter will alone give accuracy in pelvic measurement; it is a troublesome method, necessitates an anaesthesia, and a considerable amount of manipulation, but no major operation for contraction should be undertaken without its employment.

Diagram illustrative of Dr. Cameron's paper. The letter A indicates the site of adhesion.

Professor FEHLING, in reply, said: I quite agree with Professor Cameron that there are not very many cases for Prochownik's diet in pregnancy, but the practitioner may certainly obtain good results in private practice, especially in cases of slightly contracted pelvis. The question why, in Caesarean section, children have a higher mortality than premature labour children is easily answered. You make Caesarean section in married and unmarried women even for the first child. Now premature labour is induced in multiparae, and especially in married women, who have the greatest interest bringing their children up by feeding at their own breast, or a wet nurse. Hence the difference found for my clinic. I am a great friend of induction of premature labour, especially in the hands of the practitioner, and beg Professor Cameron to believe that my indications for interfering by pubiotomy or Caesarean section are as strict as his. My operated women walk after pubiotomy as easily as after Caesarean section, and I invite him to come over to Strassburg, and he will see that they are able to dance as well as others. With Dr. Munro Kerr I do quite agree as to the bad results of high forceps; my opinion is to wait as long as possible and you will have spontaneous delivery in 80 per cent. of the cases. I also agree that the operation when done on primiparae is more dangerous, because of the lacerations, but there are still cases in which it has to be done. I quite agree with Dr. Munro Kerr that the practitioner cannot avoid perforation of the living child; the clinician is obliged to save the child by pubiotomy or Caesarean section. To Dr. Tweedy I must confess I am quite an antique. After many years' use of all kinds of different pelvimeters, I have returned to the use of the hand; the first time I measure the pelvis under chloroform, and I remeasure two or three times without; then I take the average. The chief thing is the exact consideration of the labour. I only interfere when I am convinced that there is disproportion between the head and the pelvis, and that either mother or child is in danger. I cannot quite agree that the prognosis of Caesarean section should be as good as that of perforation. I am delighted to hear that Dr. Tweedy will try Bumm's subcutaneous method of pubiotomy, and am sure that in his skilled hands it will give as good results as in those of others.

DYSTOCIA DUE TO VENTRIFIXATION OF THE UTERUS.

By MURDOCH CAMERON, M.D.,
Regius Professor of Midwifery and Diseases of Women, University of Glasgow.

CASE I.

THE patient in the following case, a married woman aged 32, was admitted in her fifth pregnancy into the Glasgow Maternity Hospital on November 11th, 1908. Her previous labours had been normal and conducted without difficulty. After her fourth confinement she suffered from prolapse of the uterus, and on this account had had ventrifixation performed. About the seventh month of the present pregnancy she began to have pains like labour pains, and these continued to trouble her more or less during the last two months. True labour pains set in early on the morning of admission, and the membranes ruptured during the afternoon.

State on Admission.—She was pretty well exhausted, and the uterus was in a condition of tetanic contraction. The scar of the previous operation was seen in the abdominal wall. On examination per vaginam, the edge or margin of the os uteri could not be felt until the whole hand had been passed into the vagina. The condition was obviously one of a ventrifixed uterus in a condition of pregnancy. The os uteri pointed directly to the sacral promontory; no posterior lip of the os could be felt, the vaginal wall being continuous with the posterior uterine wall and greatly stretched. The margin of the anterior lip was close to the sacral promontory. On passing the fingers round the latter, the head could be made out lying above the promontory with the shoulder also presenting.

Craniotomy.—Under chloroform craniotomy was performed with a perforator and a crotchet. No other form of instrument could be passed. With great difficulty the roof of the skull was removed piecemeal by means of the sharp hook. By this means the rest of the head was pulled gradually into the vagina. The body had to be delivered by forcing a hook between the ribs.

Result.—After delivery a tear was found in the upper part of the posterior wall of the vagina, caused no doubt by the dragging of the posterior wall of the uterus. The tear

was only through mucous membrane, and after being examined was left alone. She was dismissed well on November 28th, seventeen days after delivery.

Patient is again pregnant, and will undergo Caesarean section at an early date.

Case II.

The patient in the second case, aged 35, and likewise in her fifth pregnancy, was admitted about a month later—namely, December 7th, 1908.

Two years after her fourth confinement she had ventrifixation performed on account of prolapse of the uterus. Labour set in on the day previous to admission. The membranes were said to have ruptured five hours previous to admission. The child was found lying transversely. The os uteri could only be felt by introducing the whole hand into the vagina as it was pointing over the sacral promontory.

Caesarean section was performed by Dr. Jardine in my absence. The adhesion band of the ventrifixation was broad and firm. The patient made a good recovery.

Remarks.

Now whilst ventrifixation has its advantages as a method of treatment for uterine displacements, the disadvantages of its seriously interfering with labour, along with the great risk of rupture of the uterus from over-distension of the posterior wall, calls for consideration as to whether this method of dealing with displacements in child-bearing women should not be modified or even extended. In such women I think the anterior wall of the organ should be chosen, and, with the patient's consent, the tubes could be tied and divided. When this is not desirable pessaries should be used to support the uterus. The relation of the parts is shown on the annexed diagram.

DISCUSSION.

Professor Jacob (Brussels) said: I am glad of the opportunity afforded me by the interesting communication of Professor Cameron to protest against ventrifixation. It should be recognized as one of the most dangerous operations in gynaecology—dangerous for young women, as Professor Cameron's case shows, dangerous for women who may still have children, and dangerous for the aged, in whom, for prolapse of the womb, it has been so highly praised. Permit me to mention three fatal cases that have come under my own observation. The first underwent ventrifixation in 1886 and died of ileus in 1891; the second, operated upon in 1890, died of ileus in 1898; the third, operated upon in 1898, died in 1901. In the first case a pedicle 9 cm. long had formed between the uterus and the abdominal walls; in the second a pedicle 7 cm. in length was found, and it was round these pedicles that the intestine had come to form the adhesions that proved fatal. The third case was one of prolapse which recurred three years after the operation, with the curious complication that the pedicle formed between the uterus and the abdominal wall dragged the wall down; there was, in fact, a true prolapse of the abdominal wall towards the vagina. In brief, I look upon abdominal hysteropexy as a dangerous operation which should not be performed save in case of necessity.

Professor Cameron, in reply to questions, explained that the ventrifixations in his cases had not been done by himself, and that he could not describe the technique that had been adopted.

DEVELOPMENT OF THE EPITHELIAL ELEMENTS IN THE OVARY.

By Annie Louise McIlroy, M.D.Glasg.,
Gynaecologist, Victoria Infirmary, Glasgow.

[Lantern Demonstration.]

Dr. Louise McIlroy demonstrated the various stages of growth of the embryonic germ cells in the mammalian ovary. The types taken for comparison were those of the pig and human fetal ovaries for the embryonic stages, and the dog and kitten for the post-natal stages. The oöcytes and cells of the stratum granulosum were shown to have their origin in the early germ cells, and the stroma cells appeared later as a framework or supporting structure only. The various forms of cell changes during maturation of the germ cell were shown and also the formation of the Graafian follicles, the research having been carried out in the physiological department of Glasgow University with Professor Noel Paton, the expenses being defrayed by a grant from the British Medical Association.

DISCUSSION ON THE TREATMENT OF THE GRAVER FORMS OF PUERPERAL SEPSIS.

OPENING PAPER.

By Thomas Wilson, M.D.Lond., F.R.C.S.,
Honorary Obstetric Officer, General Hospital, Birmingham.

For long puerperal fever was considered by different observers to be due to contagion or infection, but it has remained for the last half-century to give precision to this conception, and to demonstrate the solid foundation on which it rests. Agreement is now universal that the disease is a special case of wound infection; that it is caused by the entry of germs or their poisons at the placental site or through breaches of surface in the cervix or body of the uterus, the vagina, or the vulva; and that many different germs, streptococci, staphylococci, *Bacillus coli*, and others, are capable of producing the infection. The germs may act by themselves invading the tissues, or by giving rise to poisons which are absorbed, or in both these ways. In grave cases they probably always gain entrance from without during or soon after the process of parturition.

As the direct result of increased knowledge and improved methods of prevention based thereon, the mortality from puerperal infections in properly conducted lying-in institutions has been reduced almost to vanishing point. Outside these institutions parturient women have not shared in the benefit; upwards of 40 per cent. of the total child-bed mortality is still caused by septic diseases, from which on the average more than 2,000 women die every year in England and Wales.

Advisability of Segregation of Cases.

The mortality percentage of cases of puerperal sepsis is uncertain, but 10 is probably not far wide of the mark. At this rate there will be in England and Wales every year an average of something like 20,000 puerperal infections, and among them there is still plenty of opportunity to study and treat grave cases. Unfortunately, the cases are scattered, and only here and there provision is beginning to be made for collecting them in hospitals or other institutions suitable for research and effectual treatment. Notably in Manchester a considerable number of the worst cases are sent for treatment to the Monsall Fever Hospital, where Dr. Knyvett Gordon is making good use of opportunities of studying the treatment of these affections, opportunities that are unrivalled elsewhere in the kingdom. Throughout the country generally the rule appears to be that where patients infected in child-bed are not left to work out their own fate as best they may in their own homes, they are sent into Poor Law infirmaries, where the provision for effectual treatment is either non-existent or inadequate. The disease has been made notifiable, and there are the strongest possible reasons for the community to provide effective means for isolation and treatment, as is already done in scarlet fever, small-pox and diphtheria. At present the recognition of puerperal sepsis is made with reluctance for fear of unjust blame being imputed to doctor and nurse by the half-informed laity, and the treatment is often half-hearted and futile because of the lack of proper appliances and convenience, and of the dread of spreading infection to other patients. Prompt diagnosis followed by segregation in a properly equipped hospital is to be desired. Curative treatment would thus be made easier and therefore more effectual; the nurses would be specially trained, and the doctor have a chance of increasing his skill by extended practice under favourable conditions. The absence of such organized provision renders the investigation and study of the disease not only difficult, but incomplete and scrappy, and makes impossible a sufficiently extended practical trial of new and promising methods of treatment.

Initial Treatment.

Grave cases of puerperal fever are such as threaten the life and health of the patient. The worst cases usually

arise in the first few days after delivery, either at or near term, or much less commonly after abortion in the earlier months. At the onset it is impossible to tell whether we are at the beginning of a serious or even fatal illness, or are merely witnessing a mild and passing toxaemia. In either case the symptoms are those of fever, often ushered in by shivering or distinct rigor, and attended often, but by no means always, by lower abdominal tenderness, and by changes in the lochia. This may be scanty, suppressed, or offensive, but in the worst cases of streptococcal infection may present no obvious change in quantity, appearance, or odour. The rise in temperature is the earliest and most convenient symptom on which to decide upon treatment. So soon as the temperature is observed to be raised to 102° F., a careful examination of the patient should he made, and in the absence of an obvious and sufficient cause, such as a hard swollen breast, or a sore throat, the presumption is that infection has taken place, and this presumption should be acted upon without the delay of a single hour. Procrastination at this point is the most fatal, as it is the most usual, error. The delay of a few hours in commencing treatment may make all the difference between a slight and transient illness and a grave infection that may lead inevitably to the death of the patient.

The indications at this stage are to kill and remove the germs in the neighbourhood of the absorbent raw surfaces; to remove as far as possible all necrotic and putrescible materials, solid and fluid, and to do this without injuring the healthy living maternal tissues. The administration of a copious uterine douche by a funnel and tube through a glass Budin's tube meets these indications. Before giving it, the vulva, vagina, and cervix should be thoroughly cleansed, and, through a speculum, some of the contents of the uterine cavity obtained for bacteriological examination. This may conveniently be done by means of a glass tube 10 in. long and $\frac{1}{8}$ in. in outside diameter, or by passing into the uterus a pencil or small swab similar to that commonly used to obtain specimens from diphtherial throats. Films and cultures enable a working diagnosis of the germ implicated to be made within a few hours.

The uterus must be washed out copiously and thoroughly, at least 5 or 6 pints being used. Opinions widely differ as to the best addition to be made to the water used for injection. Plain water is not suitable, and the addition of carbolic acid or corrosive sublimate is too dangerous. Some practitioners use normal saline solution, and claim very good results. I am in the habit of using large quantities of iodine solution, which forms a reliable antiseptic, and at the same time has not appeared to give rise to any symptoms of poisoning.

Instead of, or in addition to, the use of a copious douche, strong antiseptic applications to the inside of the uterus are often employed, but with questionable advantage. It is impossible by any method of cleansing to free the uterine cavity entirely from the invading germs; and the application of caustics, such as 10 per cent. solution of carbolic acid in spirit, of pure liquid carbolic, izal, strong solutions of iodine and the like, have the disadvantage of causing the death of a certain thickness of the tissues to which they are applied. Usually, however, this method thoroughly carried out appears to be successful in causing attenuation of the germs, while the superficial necrotic layer saturated with strong antiseptic forms a sufficient barrier to the spread of the germs, until the living tissues have had time to form a protecting and separating granulation layer.

In most cases the use of the uterine douche is followed by a decided fall of temperature, together with marked improvement in the other symptoms. If the fever does not drop, further active treatment should be undertaken after an interval of not more than twelve hours; or, if the temperature rises again later, the uterus should be explored without delay. The indication is to make quite certain that no putrescible materials, snch as clots, portions of placenta or membranes, are retained in the uterus.

Digital Evacuation v. *Curetting.*

In attempting to fulfil this indication, we find practitioners fairly sharply divided into two classes—those who advocate the use of the curette and those who rely upon digital evacuation. In either case a copious intrauterine douche is indicated both before and after the intervention. An anaesthetic is usually required, either for curetting or digital evacuation, and one should be administered without hesitation. The digital method has the advantage of enabling one to ascertain whether there is any coarse débris or retained clot present in the uterus, and what is the condition of the wall of the organ, while doing little or no injury to the surface of the mucous membrane. It has been objected that if the uterine walls are stiff the finger cannot reach the fundus, an objection that rarely has any foundation in fact when the examination is made in the early stages of the disease. In favour of the curette it is urged that the removal of dead material is much more thorough than with the finger, but I have seen several cases where much coarse débris has been left behind in the uterus after the use of the instrument. It may be objected, of course, that the curette was not used in these cases by skilful hands; but in an affection so widely spread and so frequently occurring in places where assistance is not available other than that of the general practitioner of average skill, that is the better of two equally good methods which is the more easily acquired, and in this respect digital evacuation has the advantage. To the use of the curette, even in the most skilled hands, it may be objected that the scraping is applied to the whole interior of the uterus, to surfaces that may be unaffected, as well as to the diseased parts; and that new lymph and blood channels are opened up which offer the germs easy access to the deeper layers of the uterine wall. It is quite impossible to get the whole of the injured tissues away, and much less is the curette or any other method able to get rid of all the germs. It has further been objected to the curette that by its use the protective barrier of leucocytes which forms the first line of defence of the body against bacterial invasion is removed, but this argument has little weight here, since the operation we are considering ought to take place before this barrier has had time to form. Taking everything into consideration, I am of opinion that digital evacuation presents solid advantages over the use of the curette in cases of puerperal infection, and an extensive practical acquaintance with the method has confirmed me in this opinion.

General Management.

Throughout the course of the case the general care of the patient requires absolute rest in bed, together with careful attention to the diet, which, it is agreed, must be at once as nutritious as possible and easily digestible. The administration of an effective aperient is useful at the outset, and means must be taken to control any pain that may be present.

Among drugs, quinine, alcohol, and strychnine are those which, in my experience, have been of most service. Quinine, in doses of from 2 to 5 grains every four hours by the mouth or of 10 grains twice a day in the form of suppositories, has appeared to me in many instances to exert a favourable influence on the nervous system and on the body generally. In carefully regulated doses frequently repeated, and controlled by the observation of the pulse, temperature, tongue, and skin, alcohol has appeared to me to be of conspicuous service in many cases. In severe cases with failure of circulation, strychnine, especially in hypodermic injections, is often given with marked benefit. The use of antipyretics is not to be recommended. The rise of temperature appears to be a measure of the patient's general resistance against bacterial invasion. Only in very high fever does the increased temperature itself appear to harm. When the temperature is unduly high tepid sponging of the surface of the body controls the fever and at the same time comforts and soothes the patient. The sponging may be repeated with advantage whenever the temperature rises above 103° F.

When local measures, douching and curetting or digital evacuation, have failed, and it becomes probable that septic germs have penetrated more or less deeply into the tissues, the problems of treatment become much more difficult of solution. The indications still are to attempt to injure or destroy the germs and their poisons, and to encourage and reinforce the natural powers of resistance of the body.

Direct attempts to paralyse or kill the germs have been made by the injection into the circulation or under the skin of certain substances such as formalin, corrosive

sublimate, and collargol. The obvious difficulty here is to find a substance that shall be effective on the germs and still not injure the patient's tissues.

I have made trial of collargol, injected into a vein or administered by enema, in a fair number of cases, but have not hitherto satisfied myself that the drug has any definite influence over puerperal sepsis; there have been no ill results, and I think it is worth an extended trial in suitable cases. The collargol disappears from the blood stream in the course of a very few hours, and it is desirable, therefore, to repeat its administration at frequent intervals—at least twice in the twenty-four hours. It is only likely to be beneficial in a bacteriaemia or toxaemia in the early stages, when the patient's resistance is still strong. As with immune serum, so collargol cannot get into abscess cavities, sloughs, or thrombi, and therefore it cannot be expected to be of direct service in the local manifestations of septic infection. Over the serum it presents at least the advantage that its use is not necessarily limited to one variety of germ, but may be extended to all cases of bacterial invasion.

Indirect means of attacking the invading organisms include, in the first place, attempts to favour the production and activity of phagocytes, which originate chiefly in the bone-marrow, and appear to play a principal part in the defence of the body against infection. With this end in view trial has been made of subcutaneous injections of nuclein, and of the production of so-called abscesses of fixation (Fochier) by the hypodermic use of quinine or oil of turpentine. The introduction of large quantities of water or normal saline solution has been supposed to act partly in the same way, and is in any case very useful in many instances, especially in the early stages of severe infection. Large amounts of water or soda water may be drunk with advantage by the patient; if she is vomiting, half-pint saline enemata may be given every four or six hours; or subcutaneous infusions may be administered of half to one pint twice or three times a day. These are often useful in helping to support an embarrassed heart and flagging circulation; excretion from kidney, skin, and bowel is increased, and the flushing of the tissues aids in carrying off bacterial poisons and waste products.

Immune Serum.

Encouraged by the success that has apparently followed the use in diphtheria and certain other diseases of specific immune serums derived from animals, attempts have been made in the last dozen years or so to find a serum that will cure puerperal fever. The results hitherto have been disappointing.

I have never been able to convince myself that the use of antistreptococcic serum has been of distinct service in the grave cases of puerperal infection in which I have employed it. In the less rapid cases its use has occasionally been followed by obvious improvement; but in most of these also there has been no recognizable effect.

Vaccines.

More recently vaccines have been prepared from the invading organism actually present in the patient, and have sometimes appeared to give good results. Hale White and Eyre[1] claim two recoveries out of three cases of puerperal infections treated by vaccines. In a recent case of my own the patient suffered from a subacute pyaemia, of which the first effect was a suppurative panophthalmitis. The use of a vaccine of streptococcus, kindly prepared for me by Dr. Wynn, appeared to be of service after the injection of collargol, antistreptococcic serum, and other treatment had all proved abortive. Within a day or two of the first injection of vaccine abscesses began to make their appearance in the cellular tissue of various parts of the body, and as each succeeding abscess was opened a continued improvement was observed. How far the recovery of the patient, which quickly ensued, was to be attributed to the use of vaccine or to the reassertion for some other reason of the natural resistance of the patient's body it is impossible to say.

The Surgery of Puerperal Infection.

The success that has attended surgical intervention in such affections as peritonitis from diseases of the vermiform appendix, or phlebitis following on inflammations of the middle ear, has encouraged a more extended trial of various operations in puerperal infections. Experience is still too limited to allow the formation of settled opinions as to either indications or technique. Puerperal infection is of infinite and ill-understood variety, and the affection undergoes sudden and unexpected turns, so that prognosis is ever doubtful and the result of any therapeutic method hard to estimate. In the meantime a careful study of the natural course of the disease, patience, and a judicial mind are most desirable aids in the endeavour to settle the best method or methods of treatment.

In the worst cases of puerperal infection the bodily resistance, local and general, is rapidly overwhelmed by the invading micro-organisms, which spread through the walls of the uterus without hindrance into the peritoneum, and lead to the unavoidable death of the patient in a few hours or days. The expectation of good from treatment of any kind is very small, and especially is it vain to expect help from surgery; any operation is useless, and can only hasten death.

Incisions and Drainage in Puerperal Peritonitis.

In other cases, where large collections of turbid serofibrinous or purulent exudations collect with few adhesions, the patient's life may sometimes be saved by timely incision and drainage. Without surgical interference death is unavoidable, and in these grave cases the operation must be done early—in the first two or three days after infection. The indications for the operation are difficult to define. In many cases the symptoms come on with great severity, but soon moderate, the disease running a favourable course. Where there is progressive failure of strength, increasing pulse-rate, general tenderness, and distension of the abdomen, together with retching or vomiting, operation should not be delayed until the patient is too much exhausted to justify the hope of recovery. In puerperal cases operations have not been so successful as in certain other varieties of spreading peritonitis, such as those due to perforation of a gastric ulcer, or to appendicitis. In cases of puerperal sepsis the avenue of infection is by the uterus, large, soft and vascular after recent delivery; the invading germs are very virulent, and the patient frequently in a profound condition of toxaemia. As in all septic states, anaesthesia is ill-borne and should be shortened as far as possible. In proportion as the symptoms progress more gradually and as larger amounts of fluid collect and become shut off from the rest of the peritoneal cavity by adhesions, so does the chance of saving the patient by operation increase.

The best method of operating is still unsettled. Most surgeons open the abdomen either by one large or several small incisions; in the latter case an incision is made on each side of the lower abdomen, and in either case counter-openings are practised through the posterior vaginal fornix, and sometimes in the loins. In the treatment of the peritoneal cavity there is much diversity; some still flush out the peritoneum with large quantities usually of normal saline solution; others mop out as clean as possible with gauze swabs; and still others make no attempt at complete cleansing, arguing that new germs will continue to gain access from the unremoved sources of infection, and that handling the intestines increases collapse and lessens the powers of resistance, local and general. All are agreed that free drainage should be provided by the use of glass or rubber tubes, or gauze, or both. Where the intestines are distended and inactive in these cases of septic peritonitis there is greatly increased danger of toxaemia, and experienced surgeons advocate emptying the bowel by punctures or small incisions.

Another group of operators advocate early and free opening of the peritoneal cavity through the posterior vaginal fornix by the method first described by the late Dr. W. R. Pryor. This includes a thorough cleansing of the interior of the uterus, which is packed with iodoform gauze; a posterior vaginal incision is then made, through which the peritoneal cavity is examined, and adhesions about the back of the uterus and the appendages broken down; the pelvis is then carefully packed with gauze carried up high behind the uterus, with the intention of segregating this organ and its appendages from the rest of the peritoneal cavity. I have tried this method in a few cases that appeared suitable, and have been reasonably satisfied with the results obtained.

[1] *Lancet*, vol. i, 1909, p. 1589.

There are not sufficient data to justify the formation of a definite opinion as to the best treatment of puerperal peritonitis. Those cases offer the best hope of recovery that arise gradually, run a slow course, and do not fully develop until the second week. Large series of cases are to be desired in which adequate bacteriological investigations are carried out, so that the results of treatment may be separately considered for peritonitis due to each variety of form. It is well known, for instance, that in peritonitis caused by the gonococcus operations are successful, but even without operation the prognosis is, as a rule, favourable.

Hysterectomy.

In the treatment of peritonitis in puerperal cases, as in affections of the vermiform appendix, the removal of the port of entrance of the germs would at first sight appear likely to be of the greatest service, but as practical methods of treatment abdominal and vaginal hysterectomy have yielded very unsatisfactory results, and the removal of appendages infected by virulent germs has proved a very fatal operation. In any case the selection of suitable early cases for radical operation would be of extreme difficulty. The operation does not avert death in half the cases, while some of the recoveries following its performance are far from convincing. It is difficult to understand what good operation can possibly have done in a case, for instance, where the patient recovered although there were general systemic infection and septic pneumonia.

In certain definite groups of cases operation is clearly indicated. In sloughing fibroids, certain cases of rupture, and in suppuration of the wall of the uterus where this can be diagnosed, radical intervention offers the best chance of recovery. In well-marked cases of hour-glass contraction of the uterus due to septic infection, hysterectomy would appear to offer the best chance of success. A case of this kind came under my care at the Maternity Hospital last year, and the result of the *post-mortem* examination suggested that the patient's life might possibly have been saved by removal of the uterus.

Surgery of Infected Uterine Appendages.

The removal of the infected uterine appendages is not good treatment in recent cases of puerperal infection. Operation, whether abdominal or vaginal, involves the gravest risk of setting up a spreading peritonitis, while inflammation of the appendages often clears up and leaves remarkably little change behind it. It is therefore advisable to wait if possible for at least from six to twelve months before undertaking any radical operation. By this time the invading germs have died out or become much attenuated, the natural powers of recovery and repair have had a reasonable chance to complete their work, and it is thus possible to ascertain the amount of permanent damage to organs. It is not always possible to wait; in some instances suppuration about the appendages causes prolonged fever, with the attendant symptoms of severe absorption, pains, rigors, vomiting, sleeplessness, and progressive wasting and loss of strength, and it becomes desirable to intervene even in the earlier acute or septic stages of the affection. When this is necessary it is best to operate through the posterior vaginal fornix, not removing the appendages, but opening up any purulent foci that may be present in them or in their immediate neighbourhood, and providing for adequate drainage. Suppurating cysts of the ovaries must be removed, because the dangers of leaving them are greater than those of removal; the risk is comparatively small if the cyst can be delivered entire, but this is often difficult, because of the friability of the inflamed walls.

In certain cases of acute pyaemia where numerous small collections of pus containing virulent germs form in the peritoneum and in the pelvic connective tissue, early incisions for the evacuation of pus have appeared to me to do harm; this may have been due to unintentional breaking down of peritoneal adhesions, or to the opening up of new avenues to the invading germs in a large fresh wound. In such cases experience in the future may teach one to hold his hand for a time; but there can be no hesitation in agreeing that where large collections of pus have formed these should be dealt with on ordinary surgical lines, by early and free incisions in the most accessible direction, through the vagina, or by laparotomy through the middle line or through a lateral incision. The greatest care has to be taken not to open up the general peritoneal cavity, and where this is done the best plan is to pack the wound with gauze in order to promote the formation of adhesions, and not to let out the pus until after the lapse of two or three days.

Ligature and Excision of Veins.

Where infection has spread into the uterine or ovarian veins, giving rise to thrombo-phlebitis, the attempt has been made to cut short the disease by ligature or excision of the diseased vessels.

While it seems likely that this operation may be the means of saving life in carefully chosen cases, the indications for the operation are difficult to define. In acute pyaemia, and in cases where collections of pus have formed in important organs, the operation is of no avail. Fortunately a considerable proportion of patients in both these categories recover without operation. The most hopeful cases for the operation are those typical ones with up and down temperature and pulse, where several—say, five or six—rigors have occurred, and where there are few or no secondary foci, and those not in dangerous positions. When the patient's general condition is good and the pulse still strong, a carefully planned operation affords a fair chance of recovery. The best method of operating is still a matter for discussion. The intraperitoneal route is preferable to the subperitoneal, because it affords the opportunity of examining all four groups of veins. The complete removal of the affected portion of vein is to be preferred to simple ligature, but when the vein is suppurating the removal through the peritoneum is fraught with danger of general peritonitis.

In cases of pyaemia many other operations have to be performed—incision and drainage of superficial abscesses and of empyema, the opening of localized collections of pus in the peritoneum or in joints, the amputation of limbs in cases of gangrene, and others. These call for no special consideration in a discussion like the present.

The curative treatment of grave cases of puerperal infection is not, and from the nature of the process probably never will be, satisfactory. Hope for the future must still centre round prevention. The methods of prevention are known, and a more general and intelligent application of them by a better trained generation of nurses and doctors may reasonably be expected to bring about the practical abolition of grave cases of puerperal sepsis, just as spreading traumatic gangrene has been banished from our hospitals.

DISCUSSION.

Professor Fehling (Strassburg) said: I am happy to be able to agree with Dr. Wilson in almost all that he has said. I wish to bring forward one point—that is, that the best preparation for the successful treatment is exact diagnosis; but bacteriology does not yet enable us to ascertain with certainty whether we have to deal with haemolytic or anaemolytic streptococci; moreover, haemolytic streptococci may become dangerous, and it seems possible that anaemolytic streptococci may become haemolytic and vice versa. If that is possible *in vitro*, it will also be possible in the interior of the uterus. The second difficulty is that, though streptococci in the uterus always indicate severe disease, a patient may die without streptococci in the blood or survive in spite of their presence. I am of opinion that, for purposes of treatment, a strong distinction must be drawn between cases of sapraemia or toxaemia and streptococcic infection. As regards internal treatment, I agree with Dr. Wilson that neither antistreptococcic serum nor collargol (intravenously or per rectum) has been of much value, though in favourable cases the pulse-rate and temperature diminish. The treatment of streptococcic infection with vaccine is a different matter, and we have begun to give vaccine in severe cases and also in pregnancy prophylactically. It is too soon to give results of this treatment. On one point I cannot agree with Dr. Wilson—that is, the internal examination of the uterus in the first week of the puerperium. I think it dangerous and of no value. The risk of implanting germs in the uterine wall is too great compared with the advantage. Concerning the washing out of the uterus, I

do not agree with Professor Winter, who, in the recent Congress of German Gynaecologists, condemned it altogether. In cases of high fever I take the secretion out of the uterus for examination. If we find only saprophytic germs or *B. coli*, the fever falls after the intrauterine irrigation, but not so when there are streptococci; so we have in the result of bacteriological examination and irrigation a point for our prognosis. Pieces of placenta should always be removed; but curetting in cases of septicaemia is dangerous after full-term delivery; it is only allowed in cases of sapraemia after miscarriage or sapraemia in the second week. Concerning the operative treatment, we must confess that our results are not satisfactory. The extirpation of the uterus gives a mortality of more than 50 per cent. If streptococci are in the blood the extirpation of the womb cannot be sufficient. The operation in cases of peritonitis seems to have more effect, especially if it is done early. I prefer one medial incision and two in the flanks, with introduction of drainage tubes, but without washing out. It is easily to be understood that the opening of local peritonitis and the extirpation of suppurating tubes and ovaries will give good results if not undertaken too soon. Still, it is very difficult to decide when operation should be done in cases of thrombo-phlebitis followed by pyaemia. Bumm recommends us not to operate in the acute stage, more in the chronic, and only to ligature the thrombosed or purulent vessels. There are certainly some good results. We can only hope to have better results in the future by the better instruction of students and midwives, and by the invention of bacteriological and serum methods which will enable us to make the diagnosis of septicaemic infection much sooner.

Professor Murdoch Cameron (Glasgow) said: I have never seen death from sapraemia in any case seen early and properly treated by the removal of all débris, swabbing out the cavity, and then swabbing it out with camphor and carbolic acid—crystals of carbolic acid rubbed down with sufficient camphor to liquefy them. In true septicaemia I have had very good results from serum treatment. The important point is to train our nurses, midwives and students in aseptic and antiseptic precautions.

Dr. Munro Kerr (Glasgow) referred first of all to the difficulty in deciding how soon the uterus should be explored when there were manifestations of sepsis. One saw many cases improve with merely raising the foot of the bed and giving vaginal douches; consequently he thought that the uterus should not be immediately explored unless there was some débris left, and the uterus was infected. The former condition might be suspected if the placenta or membranes did not come away easily and entire, while the second condition should be suspected if the hand had been introduced into the uterus during parturition, and could be decided by the removal of some material from the inside of the uterus with a swab for bacteriological examination. If, after expectant treatment, no improvement followed, then the uterus should be explored. Curettage of the uterus he was opposed to, preferring to remove any débris with the fingers and gauze, and wash out the cavity with some antiseptic solution. He had not found plugging with antiseptic gauze to be followed by any better results. It was difficult to decide when to operate in the graver forms of sepsis, for so many of these serious cases ultimately recovered. There was, however, he considered, one type of cases always most grave, and nearly always fatal, namely, acute peritonitis. This type fortunately was very rare. In such cases drainage of the peritoneum or removal of the uterus might result in benefit. Cases which went on for weeks should be treated surgically. Treatment with antistreptococcic serum he found of no value, but vaccine treatment was a different question. At present one could say that chronic cases would certainly often be benefited by vaccines, but it was still impossible to say that acute and subacute cases would be at all improved. The prophylactic vaccination against septicaemia referred to by Professor Fehling was a matter of very great interest.

Dr. Hastings Tweedy (Dublin) said: It is a mistake to consider that sepsis has been abolished in maternity hospitals. It is still seen, and we encounter large numbers of cases of curable sepsis. It is curable because we start treatment at an early stage, and do not wait, as suggested by Dr. Wilson, until temperature reaches 102° F. To wait so long is to waste valuable and all-important time. Incision through the posterior fornix is at once the simplest and most satisfactory method of draining the abdomen. The insertion of iodoform gauze through this incision will cause isolation of the pelvic organs from those of the general peritoneum. An efficient drainage will also be established, and this with infinitely less shock than that which is produced by abdominal section.

Dr. Frederick Edge (Wolverhampton) said: The general practitioner is blamed for puerperal sepsis, but really it is the professors of midwifery who are to blame for their lack of simplicity and clearness in instructing him as to the dangers of touching the cavity of the uterus after the placenta and clots have cleaned it during their expulsion. In other words, he is not taught the management of the third stage of labour. The germs in the vagina become from want of cleanliness more virulent and haemolytic after some hours or days, and any manipulation hours or days after the labour is over is correspondingly dangerous. Such a manipulation is douching when the vulva and vagina have not been cleansed as a preliminary. Most medical men conduct the confinement quite successfully, but occasionally one gets nervous and spoils a good job by unnecessary interference. My routine practice is to clean out the uterus with finger and *blunt* copper curette, then wash out with normal saline solution, and drain by packing with iodoform gauze. When a case is septic I open the pouch of Douglas and pack with gauze. There is always some sero-purulent fluid to come away and abscesses. The patient should be placed with her trunk elevated (Fowler's position). As soon as possible public provision should be made for these cases, both in respect of hospital accommodation and also as to vaccines, so soon as these have been worked out better.

Professor Alfred Smith (Dublin) confined his remarks to the purely operative treatment. For puerperal peritonitis he advised opening the abdomen by an extensive incision. He places a large drainage tube in the upper angle of wound, another in the lower angle, and with a modification of Moynihan's continuous rectal irrigation he causes normal saline at a temperature of 100° F. to flow continuously through the peritoneal cavity—if necessary openings may be made in flanks or posterior cul-de-sac; this continuous use of saline prevents the formation of adhesions around the drainage tube and encourages free drainage. In regard to thrombo-phlebitis, he emphasized the difficulty of differential diagnosis, and drew attention to the importance of the leucocyte count. With regard to the position of hysterectomy, he was not in a position to dogmatize, but thought badly of it both as a curative and prophylactic procedure. He advocated prophylactic removal as best for the patient if the pathologist proved the presence of haemolytic streptococci associated with placental débris.

Dr. Cuthbert Lockyer (London) stated that his custom was first to obtain a swabbing from the fundus under strict antiseptic precautions; he then explored the uterus with the gloved hand, and removed any products of conception if they were present. Next, instead of curetting, as he used to do, he contented himself with firm swabbing or brushing of the uterine wall with gauze wrapped around forceps and steeped in a strong iodine solution. The uterus was then drained by 10 per cent. iodoform gauze, and ernutine and trianine injected hypodermically. From the swabbing Dr. Eastes provided a diagnosis of the organism within an hour of its being sent to the laboratory. If the *Streptococcus pyogenes* was the predominant organism in the smear, acting on Dr. Eastes's advice, the speaker was in the habit of injecting 30 c.cm. of polyvalent serum, giving any subsequent dose by the bowel to obviate any of the untoward sequelae—for example, irritative rashes, joint pains, or synovitis. In about forty-eight to fifty-six hours a vaccine was made, and in the case of streptococcal infection administration was begun by a minimal dose of $\frac{1}{10}$ million, given during a positive opsonic phase (in cases where the index had been taken) or during general improvement as

estimated clinically where the index could not be taken. The dose of $\frac{1}{10}$ million was followed by $\frac{1}{5}$ million, and so on. In one grave case, for example, the doses were: June 25th, $\frac{1}{10}$ million; June 30th, $\frac{1}{5}$ million; July 7th, $\frac{1}{4}$ million; July 13th, $\frac{1}{4}$ million; July 19th, $\frac{1}{2}$ million. This was in a case where streptococcal pus formed in the right knee-joint, and the woman was now (July 28th) making a good recovery. In conjunction with Dr. Eastes he had used vaccine in 4 cases with markedly good results. In a fifth case no vaccine was made, the blood being sterile, but the woman was given in all 150 c.cm. of polyvalent serum, and recovered without any local lesion. With regard to drainage by the pouch of Douglas, he had twice had to do this for definite indications (retrouterine swellings), but saw no reason to do so as a routine method of treatment. Ligature of veins might do good in cases which ran a chronic course with recurring rigors, but it was difficult to choose the right cases for this operation. In one case, in which the patient lived for fifty-six days after delivery, there were repeated rigors, and the patient ultimately died of multiple emboli in the lungs. At the *post-mortem* examination a suppurating thrombus was found in the right pampiniform plexus of veins. This would have been a suitable case for ligation of the pelvic veins if the right time had been chosen for operating. It was his practice to use subcutaneous injections of saline solution, believing that the toxins were diluted and their elimination favoured thereby. He believed in the administration of large doses of perchloride of iron, but found quinine of little value. Vaccine treatment had a future. It was a pity that in London there was no adequate provision made for the reception and treatment of puerperal sepsis in any institution. If the cases of puerperal fever met with among the poor were admitted into an institution where the services of a bacteriologist and opsonist were available, many cases which now proved fatal (when dealt with by clinical methods alone) would recover.

MIDWIFERY IN PRIVATE PRACTICE:

A CRITICISM OF METHOD BASED ON ACTUAL RESULTS.

By JOHN J. AUSTIN, M.D., M.Ch.R.U.I.,

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IN bringing the subject of midwifery in private practice before you to day, I do so from the standpoint of one engaged in general practice. The views expressed, though they may differ in certain respects from present-day teaching, are based entirely on the results of my own cases. Never having seen a report of cases treated in private practice, I began a few years ago to keep a record of my own cases, that I might know what results could be obtained and how they would compare with those treated in a well-equipped hospital, where all arrangements are perfect and the treatment of the patient is under complete control after admission.

My cases were all attended while I was engaged in the ordinary routine work of a general practitioner. No case was refused because it might turn out unusually difficult or dangerous, nor was attendance on any case declined because I happened at the time to have infectious or suppurating cases under my care.

My present series, consisting of 465 consecutive cases, includes 29 miscarriages, 9 breech presentations, 1 placenta praevia, 1 *post-partum* haemorrhage, 1 accidental haemorrhage, 1 eclampsia, 1 induction of premature labour owing to pelvic contraction, and 5 adherent placentae. There were no deaths among the mothers, and all made good recoveries.

Temperatures.

The temperatures do not include those of the first twenty-four hours after delivery, and were not taken after the seventh day. They were all taken by myself, sometimes in the mouth, but more frequently in the axilla. They were taken at all periods of the day, as suited my own convenience. Of the 465 cases the temperature in 358 (77 per cent.) did not rise above 99°. In 64 cases (13.7 per cent.) the highest temperature reached a point between 99° and 99.5°; in 22 (4.7 per cent.) between 99.5° and 100°; in 17 (3 6 per cent.) between 100° and 101°; in 3 (0.6 per cent.) between 101° and 102°. In one case (0.2 per cent.) it reached 103.8°. In the latter case the patient's temperature had been normal during the first six days, and on the seventh day it was 103.8°, the pulse being 96. At this visit I observed the nurse had an inflamed and swollen hand, resulting from a punctured wound. She was sent as quickly as possible to the Royal Victoria Hospital, where she was detained under treatment for about two months. The patient's temperature and pulse remained 103.8° and 96 respectively until the twelfth day, when they both fell to normal. In the other cases the rises of temperature were only temporary, many being normal next day, and all were normal in two days without any treatment, except in one case where the patient suffered to an unusual degree from engorgement of the breasts, accompanied by pain and tenderness. This case was normal in three days. In 31 of the cases where the temperature was elevated, the patients suffered considerably from engorgement of the breasts, the result of commencing lactation, while in 23 other cases where patients were suffering similarly the temperature was normal.

Miscarriages.

Twenty-nine of the cases were miscarriages occurring between the second and seventh month of pregnancy. In 18 of these a sharp flushing curette was used to remove the ovum, either in whole or in part. In one case occurring at the fifth month the fetus had been dead for ten or twelve days, while in two others occurring in the seventh month the fetus was believed to have been dead in each case for two months prior to expulsion. All the patients made a good recovery. The highest temperature recorded in any of these cases was 99°. The separation and removal of an abortion by the finger-nail is a feat which I have never been able to accomplish satisfactorily, and until adopting the curettage treatment I seldom or never left an early abortion case with an easy mind.

After deducting the miscarriages there remain 436 cases at term, 308 being multiparae and 128 primiparae. Of the 308 multiparae only 4 (1.3 per cent.) were delivered by forceps, the remainder (98.7 per cent.) naturally. Of the primiparae 34 (26 56 per cent.) were delivered by forceps and 94 (73.43 per cent.) naturally. By way of contrast I might mention that in the first 436 full-term cases I attended after commencing practice there were 103 (23 61 per cent.) forceps cases, as compared with 38 (8.71 per cent.) in the present series.

While I think the statistics I have given show that I could never have been accused of being in a hurry to apply the forceps, I am certain that we have been in the habit of using forceps more frequently than was either necessary or advisable. I am convinced that many of the difficult forceps cases met with are of our own making; that all the patient requires is a little time and she will deliver herself. Many cases delivered by forceps with the head in the occipito-posterior position would rotate and be easy natural cases if time were allowed. Cases are not uncommon in which attempted delivery by forceps failed, and in a comparatively short time afterwards the patient delivered herself unaided. The too common practice of giving an anaesthetic and applying forceps long before the maternal parts are ready for such treatment is much to be deplored. When pains are weak and labour is progressing slowly, instead of the application of forceps the administration of 10 grains of quinine will frequently bring about the desired result. Quinine, like ergot (which used formerly to be given for feeble pains), sometimes fails to produce any effect; but, unlike ergot, it does not cause tonic contraction of the uterus, but merely strengthens the natural pains and facilitates, rather than hinders, the expulsion of the placenta. It may be safely given, in my experience, even in primiparae before the cervix is fully dilated.

Perineum.

The condition of the perineum after delivery was carefully examined in each case, and the result forms another argument against delivery by forceps. Many lacerations occurred in multiparae as well as in primiparae. Each laceration, however small, was sutured with silkworm gut and healed satisfactorily. When done with due antiseptic precaution, care being taken to remove all blood clot, this is one of the most satisfactory operations in midwifery. Besides adding greatly to the after-comfort of the patient,

this practice largely diminishes the risk of sepsis. The sutures were removed about the tenth day.

In primiparae the perineum was torn in 86 cases (67.2 per cent.), and not torn in 42 (32 8 per cent.). Of the cases with lacerations, 30 (34.8 per cent) were delivered by forceps, and 56 (65.2 per cent.) naturally, while of the 42 cases not torn, only 2 (4.7 per cent.) were forceps, and 40 (95.3 per cent.) natural. In a number of the cases recorded as not torn, there was a laceration of the inner or vaginal surface, though externally the perineum seemed to be intact. These internal lacerations were sutured with catgut.

Eclampsia.

One patient, a primipara, developed eclampsia a few minutes after the birth of the baby. I had only been engaged to attend her late in the evening, and labour set in early the following morning, so that I had no opportunity of using prophylactic treatment before labour, though she had been suffering from marked toxaemic symptoms for a month before delivery. When a patient, especially a primipara, comes to engage me, I ask her to inform me at once if she notices the urine becoming scanty and dark coloured, or if the ankles swell, or if she suffers from headaches or dimness of sight. If I have reason to be specially apprehensive, I teach the patient or her husband to examine the urine, and request that this should be done at short intervals. In this way I have often got threatening cases under treatment early, with the result that in no case except one did eclampsia ensue when I had seen the patient before she was in labour. In the one exception the patient had no sign whatever of toxaemia; I observed that she had hardly any loss of blood at the birth, the draw-sheet having been scarcely soiled. The late Dr. Fordyce Barker described two similar cases where the patient had scarcely any loss of blood at the birth, and each developed eclampsia. I look on eclampsia as being quite as preventable as puerperal fever.

Post-partum Haemorrhage.

One patient, a primipara, who had been delivered naturally, showed a decided tendency to *post-partum* haemorrhage. It was difficult at the time to understand why this should have happened, as it is of rare occurrence in primiparae. A few days after delivery the nurse informed me that when this patient was born her mother died of *post-partum* haemorrhage. I have met with the same condition in several patients whose mothers had died of *post-partum* haemorrhage.

Routine Treatment.

In working out the routine treatment, which, with slight modifications, I have adopted for the past fourteen or fifteen years, my aim was to arrive at a method which could be carried out by any general practitioner, and which would eliminate the risk of all preventable complications, and leave the patient at least as well as she was before conception occurred. During the last few weeks of pregnancy my patients are prepared for the approaching event by means of a tonic consisting of:

℞ Tr. ferri perchlor.	... ...	♏xv
Quiniae sulph. ...	... ...	gr.j
Liq. strych. hydrochlor.	... ...	♏iij-v
Glycerine	... ...	ʒss
Tr. aurant.	... ...	♏v
Aquae	... ad	ʒss

To be taken three times daily in water after food. Besides counteracting the tendency to anaemia, it greatly lessens the chance of tedious labour, and decidedly diminishes the risk of *post-partum* haemorrhage. Patients have frequently told me of the benefit they had derived from this treatment. It also seems to have quite as beneficial an effect on the fetus as on the mother. When labour sets in the external genitals and surrounding parts are cleansed by the nurse, and bathed with a 1 in 2,000 sublimate solution. The inner surfaces of the labia and the lower part of the vagina are wiped with gamgee dipped in sublimate solution before each examination. My own hands are thoroughly cleansed with soap and water, and soaked in biniodide solution 1 in 1,000 before each examination.

The first half of this series of cases were examined with the naked hand, in the latter half a sterilized rubber glove was worn on the right hand. Although it made no appreciable difference in the results obtained, I prefer wearing the glove owing to the greater feeling of security. The present-day tendency to regard the making of vaginal examinations as harmful to the patient does not agree with my experience. The rules laid down by teachers with regard to vaginal examinations, however well they may be adapted to hospital practice, are quite impossible in private practice. A busy practitioner must know at regular intervals of the progress of his patient, in order that he may allay the anxiety of the patient and her relatives, and also may best economize his own time. Consequently, he must make examinations as frequently as may be deemed necessary, and, if carried out on the lines already indicated, this may be done with absolute safety. The vast majority of my cases were examined only by myself. Until quite recently the nurses were not allowed to make examinations; now, when in doubt, they are permitted to do so, wearing a rubber glove sterilized immediately before use.

On completion of the third stage of labour each patient is thoroughly douched with a 1 in 4,000 or 1 in 5,000 hot sublimate solution, beginning with the vulva, then the vagina, and lastly the uterus up to the fundus. As a precaution against the introduction of air or lotion into the Fallopian tubes or uterine sinuses the left hand of the practitioner should firmly grasp the uterus while it is being douched. Having douched all my cases for the past fourteen or fifteen years I am perfectly satisfied as to its beneficial effect, notwithstanding the fact that it has, for a time, gone out of fashion with obstetricians. It has a cleansing and soothing effect upon the bruised and overstretched or lacerated tissues of the genital tract, and helps to establish the circulation which may have been long impeded through pressure of the presenting part. The washing away of large clots which frequently remain *in utero* saves the patient much after-suffering, and lessens the risk of *post-partum* haemorrhage and sepsis. Cases which have been examined without due precaution being taken have still more need for the douche. I have been repeatedly told by nurses that after being douched the patient and the bed are much more easily kept clean. I use an ordinary Rotunda douche with a uterine tube made to my own pattern by Young of Edinburgh. The curve is such as to allow of its easy introduction into the uterus without the aid of a guiding finger. It has a single large opening at the end and has no return channel. This allows of better irrigation of the vaginal and uterine walls. Any lotion remaining should be expressed. The douching is not repeated, the nurse being instructed merely to cleanse the external genitals. The douche must, of course, be thoroughly sterilized before use.

Adherent Placentae.

The placenta was adherent in five cases. A sterilized rubber glove was worn during removal in the last two cases. It is in cases where the placenta has been adherent and has required manual separation that the good effects of sublimate douching are most readily seen. Since adopting the douche as a routine the temperature in these cases rarely exceeds 99°, while formerly it was, as a rule, much higher.

Nursing.

Next in importance to leaving the patient in an aseptic condition after delivery is the securing of a reliable nurse. For many years I have tried as far as possible to have my patients under the care of the same nurses, and to keep these nurses employed all their time among my own cases. Until I succeeded in carrying this out I could not obtain uniformly good results. I must be satisfied with the nurse, and so must a large majority of her patients. Nothing conduces more to the wellbeing of a patient than to know that everything is going on smoothly in the house. If a nurse upsets the household arrangements, or worries her patient with household affairs, the patient is not likely to have a normal temperature, or make so good a recovery. It is not essential that the nurse should be highly trained. When I did a considerable amount of midwifery amongst the working classes I had a few nurses of the "handy woman" type, whom I selected and trained myself. These women were taught how to clean their hands, sponge a patient, bath a baby, and give an enema when necessary, but they were not allowed further to interfere with a patient. I taught them to judge the stage of labour by the character of the pains, and after a little

practice it was surprising how seldom I was needlessly summoned. Their cases did as well as those in which a trained nurse was employed. They nursed only my own cases. What I object to most of all is a nurse being present at the birth, and coming morning and evening to attend to mother and child. Such a person frequently has several cases on hand at the same time and then trouble begins.

To the care and attention bestowed upon the patients by the nurses and to the intelligence with which they carried out instructions I attribute in no small degree the results that have been obtained, and which I venture to submit show that, provided the measures I have suggested are adopted, as good results can be obtained in private practice as in the best equipped maternity hospital.

DISCUSSION.

Dr. HASTINGS TWEEDY (Dublin) said that Dr. Austin's paper was of great value, and about the first statistics that had been given in respect of private practice since the days of McClintock. He hoped that Dr. Austin would adopt the Morbidity Table of the British Medical Association, and at some future period show the results of his practice based on this criterion of morbidity.

UTERINE CANCER COMPLICATING PREGNANCY.

By CUTHBERT LOCKYER, M.D., F.R.C.S.,

Senior Officer to Out-patients, Samaritan Hospital; Obstetric Physician to Out-patients, Great Northern Central Hospital, etc.

My personal experience of cervix carcinoma as a complication of pregnancy and the puerperium is limited to four cases, two of which may be dismissed in a few words as they came to me after abortion in the one case, and labour at term in the other, therefore from the obstetric point of view my own observations were nil, and the cases were dealt with as ordinary cervical carcinomata.

CASE I.

A. T., aged 31 years, married nine years, two children, four abortions, the last two months ago at sixth week of gestation. Admitted into Great Northern Central Hospital on February 25th, 1907, complaining that three months before whilst pregnant she noticed a bad-smelling brown discharge, and also haemorrhage on coitus. A month later the patient aborted and the discharge became more profuse. Pain in the left side of the pelvis and frequency of micturition followed, then rapid loss of flesh. On examination, the cervix was expanded into a flat fungating mass, the size of a five-shilling piece, the growth extended also on to the vagina where it covered two square inches of the right and posterior walls. The growth was very haemorrhagic and the discharge very offensive. On February 27th, 1907, the growth was cut away and its base cauterized, and 2 per cent. formalin tampons inserted. On March 2nd I removed the uterus and upper part of vagina by Wertheim's method. The uterus was adherent to bowel and omentum. No glands were found. Time of operation 1 hour 20 minutes. April 15th: Patient quite well. March, 1908: Recurrence.

In this case the cancer was probably present at the time of conception, the abortion was a probable result of the cancer. The extensiveness of the disease was a probable result of the pregnancy. The early recurrence (one year) may be explained by the fact that the cancerous vaginal wall was torn by traction before the operation was concluded.

CASE II.

The patient was delivered on January 4th, 1909, after very severe *ante-partum* haemorrhage due to placenta praevia; this necessitated forcible dilatation and turning with extraction; the patient nearly died of haemorrhage. Examination after the puerperium was over revealed some induration around the cervix which was thought to be parametric cellulitis, then the patient was not seen again for six weeks, as her doctor went away. On second examination she was immediately sent to Mr. Targett at Guy's Hospital, who kindly handed the case on to me at once, as he had no bed vacant and considered the condition urgent, if not actually hopeless. On admission into the Samaritan Hospital the patient, a woman of 33 years of age, gave the history of losing ever since, and for a month before her confinement. I found the cervix almost eaten away, leaving an excavating ulcer extending to the anterior vaginal wall; both parametric and utero-sacral folds were infiltrated. I decided to explore, but as the cancer had extensively involved the base of the bladder I closed the abdomen without attempting the removal of the uterus.

CASE III.

On July 30th, 1903, I was called in consultation to see a lady (Mrs. C.) who had been in strong labour for fourteen hours The patient was a multipara, aged 38 years. The doctor's diagnosis was placenta praevia, but what was taken for placenta proved to be a mass of cancer blocking the vagina completely. The patient was extremely collapsed, the pulse-rate varied between 120 and 140 beats per minute, the expression was anxious, and delivery *per vias naturales* was impossible. I removed a healthy male child by Caesarean section, and as the patient was *in extremis*, I considered it best to simply remove the placenta, inject ergotin, sew up the uterus, and close the abdomen. Removal of the uterus was out of the question as the growth extended into both broad ligaments and above the bladder reflection fixing the lower segment firmly in the pelvis, and a Porro's operation would have prolonged the technique and added to the shock. The patient made a good recovery and was able to get about at the end of the puerperium, but died from exhaustion a few weeks later. The child lived and was strong and healthy. The error in diagnosis of mistaking a cancerous mass for placenta praevia has been made elsewhere. Lovrich of Budapest mentioned a case in the *Zentralb. für Gyn.*, 1904, p. 781, in which carcinoma cervicis was mistaken for placenta praevia, and a living child obtained by turning and extraction, and the uterus not removed.

With regard to employing conservative Caesarean section for carcinoma cases I feel that it is not so good as the Porro-Caesarean, and in future I should perform the latter operation on cases at term, unless, as in the case just described, the patient was *in extremis*, when I should leave the uterus and take the risks of bad drainage rather than those of increased shock.

CASE IV.

E. M., aged 32 years, was admitted into the Samaritan Free Hospital for Women on November 11th, 1908.

History.—She was thirty-six weeks pregnant with her sixth child. She had five children born alive; the first two were delivered spontaneously, the last three with the aid of forceps. There had been no puerperal complications. After the birth of the last child catamenia was regular for a year. No proper period had been seen since March 19th, 1908. Haemorrhage per vaginam started on July 16th and lasted three or four days. On August 4th there was a further loss of blood, and ever since July there had been an offensive watery discharge. No haemorrhage after coitus had been noticed. The patient went from London to Kingston in October, and whilst there had a severe flooding which necessitated her admission to Kingston Infirmary, where Dr. Pattison Armstrong removed a large cauliflower mass from the anterior lip of the cervix. There was a good deal of haemorrhage from the cervix, and Dr. Armstrong thought of the possibility of placenta praevia. Dr. Donald, the senior officer, sent for me with a view to immediate interference, and I first saw the patient on the evening of October 7th, about twenty hours after the removal of the growth. The bleeding had ceased; the womb was the size of a twenty-eight weeks' pregnancy. The surface of the cervix was smooth; no growth was discoverable; it felt as if the entire anterior lip had been cut away. The patient was very anaemic from recent haemorrhage; otherwise her condition was good. I examined the cauliflower mass attached to the anterior lip which had been removed by Dr. Armstrong, and came to the conclusion that there was no immediate need for interference, and that provided there was no sign of the growth recurring the case might be left to go to term for the sake of the child. Accordingly the patient remained in the Kingston Infirmary for five weeks after amputation of the cancerous lip of the cervix; she was then transferred to the Samaritan Free Hospital on November 11th.

State on Admission.—During the five weeks' interval there had been no fresh haemorrhage and no discomfort; the patient's health had improved—that is, she had lost her anaemia. On examination it was evident that the growth had returned. There was a mass on the left side of the external os the size of half a crown. It was a soft sessile plaque, which could not be described as haemorrhagic on examination, since there was only a slight brownish stain on the finger on examination. After a consultation with my colleague, Dr. Hubert Roberts, it was decided to operate forthwith, for, although the child was small in comparison with the dates given, interest in the mother's condition, seeing that the cancer had started to grow again, outweighed further consideration for the child.

Operation.—Caesarean section, followed by panhysterectomy by Wertheim's method, was performed on November 17th, 1908, in the presence of Dr. Amand Routh, Dr. Oxford, Mr. Butler Smythe, Drs. Donald, Frost, and others. I was ably assisted by my colleagues, Dr. Hubert Roberts and Dr. Clifford White. The operation took one hour and twenty-five minutes. The median incision was 9 in. long. The uterus was brought out of the abdomen, the intestines packed back. The ovarian vessels were clamped, the uterus incised, fetus removed, the cord divided and returned into the cavity of the uterus; the latter was then rapidly sewn up with thick-linen thread without the placenta being disturbed. The uterus was now wrapped in gauze and held over the pubes. The ureters were easily found where they crossed the iliac vessels, from whence it was noted that they ran across the pelvis at a higher level than usual. The vagina and parametric tissues were much more easily dealt with than is usual in a non-pregnant case. Every structure came up within easy reach, and the entire operation was carried out in comfort. In transfixing sutures considerable care was necessary in passing threaded needles owing to the large size of the veins.

Result.—The patient bore the operation well. The child required artificial respiration in a warm bath, but soon revived. One tube of ernutin, injected immediately after removal of the child, seemed to produce but little effect on the body of the uterus. No catheterization of the bladder was needed after the operation. The child died twenty-five hours after birth. The mother's convalescence was afebrile and uneventful, and she was discharged twenty-three days after operation. The cancer was squamous epithelial in type. There was no recurrence when last seen in May, 1909.

It will have been observed that in the Caesarean section I eventrated the uterus and clamped the infundibulo-pelvic folds before incising the anterior uterine wall. Both these steps are open to criticism. I notice that many, if not most, operators prefer to leave the fundus in the abdomen whilst the incision for the delivery of the child is made, but it seemed to me that it would be easier for my assistant to control the uterine vessels with the uterus outside the abdomen, and I cannot think that such a procedure causes an increased risk to the child. The clamping of the ovarian vessels may have been unwise, and as the child was born in a state of asphyxia and died twenty-five hours later it may be argued that it suffered from my operative technique prior to delivery. This is a point on which I invite the opinion of those present. E. Schroeder, of Königsberg, delivered a premature child by vaginal Caesarean section after ligaturing the uterine arteries at the bases of the broad ligaments. The fetal heart was only just appreciable, and the child was practically stillborn; he attributed the fetal death to the ligation of the arteries, and I am equally ready to admit that the clamping of the mesosalpinges in my case contributed to my disappointment as regards the child, although delivery was effected a few seconds after the ovarian vessels were clamped. It was my first experience with such a case, and I was very anxious to spare the mother a loss of blood which would have added shock to a prolonged dissection of the pelvic connective tissues and the vagina, such as is entailed in Wertheim's technique.

There is one more point, perhaps the most important of all, in which it may be fairly argued that I departed from the correct rules of surgery—namely, that I did not operate on the case directly I saw the patient. It is an accepted surgical canon never to delay operation in a case of operable cancer complicating pregnancy. On the other hand, Schauta mentions a case from Innsbrück in 1883 in which a woman five months pregnant had a carcinoma of the cervix limited to the anterior lip. The carcinoma was excised and the diagnosis confirmed. Normal delivery occurred at term; a healthy male child was born. Operation for recurrence took place on October 16th, 1886, and the patient was quite well twenty years later (1906). Schauta states that he would not break a lance in favour of excision of carcinoma in pregnancy, but adds, "One ought not *without exception* sacrifice the whole uterus in a case of early carcinoma in pregnancy." In my case the excision had been so thorough that I could not have conjectured the presence of recent carcinoma had I not been shown the tissue removed. I arranged for the close observation of any sign of recurrence, and as soon as it appeared I operated. Had there been no sign of return of growth I should have allowed the pregnancy to go to term as Schauta did in his case. In spite of the good result in Schauta's case, v. Franqué stated that he would not have taken such a risk, and there may be those present who feel the same. I admit that I shall have grave misgivings on this point if my patient gets a recurrence, especially as I was finally defeated in the object for the delay by losing the child on the day after its birth.

This is the first recorded case in this country and America in which Wertheim's total hysterectomy has been combined with Caesarean section. It has been successfully carried out since my operation by my friend Dr. Gray (in the absence of Dr. Herbert Spencer) at University College Hospital; and by the courtesy of Dr. Herbert Spencer and Dr. Gray I am permitted to mention that on April 26th, 1909, Dr. Gray successfully performed the Caesarean-Wertheim operation for a patient who had been thirty-six hours in labour, for whom forceps had been used and abandoned (by a general practitioner) on account of haemorrhage from an obstructing carcinoma of the anterior lip of the cervix. This growth presented at the vulva on admission. There was some difficulty in delivery of the child owing to its head being fixed in the pelvis. Moreover, the vagina could not be drawn up satisfactorily, as traction simply stretched the lower uterine segment; accordingly a portion of the vagina had to be removed four days after operation by the galvano-cautery. The patient suckled her child from the first, and was quite well when last seen on June 29th, 1909.

Prior to these 2 cases of Caesarean-Wertheim operations for carcinoma cervicis Sarwey last year put on record 13 others. All the mothers recovered; all the children were born alive. One (Micholitsch's) case has remained well for five years. Two cases suffered from vesico-vaginal fistulae.

For some years past the favourite method of dealing with cancer in the later months of pregnancy has been by vaginal Caesarean section, as recommended by Czerny and Dührssen, Olshausen, Wilson, and others. At the Oxford meeting in 1904 Dr. Munro Kerr described vaginal Caesarean section and subsequent removal of the uterus per vaginam as the most up-to-date method, but added that he did not think the method suitable when the disease is at all extensive, owing to risk of implantation and the difficulty in pulling down a friable cervix, whilst the child is placed in greater danger. The method favoured by Dr. Munro Kerr in 1904 was Zweifel's plan of carrying out classical Caesarean section, supravaginal amputation, followed by removal of the cervix per vaginam.

At the Leicester meeting in 1905 Dr. Thomas Wilson recorded 2 successful cases of vaginal Caesarean section in which both mothers and children were spared, but in one recurrence descending low into the vagina took place eleven months later, although the growth was polypoid and localized at the time of operating. In Dr. Wilson's second case the primary result was good, but the subsequent history is not given. Dr. Wilson states that decision as to the route taken in operating will depend upon the method generally adopted by the operator in cases of cervical carcinoma generally. He regarded the risk of implantation by both routes as equal, but this was at a time when we in England had no personal experience of Wertheim's technique, which is specially devised to obviate the infection by cancer cells of newly cut surfaces. Among other points advanced by Dr. Wilson in favour of the vaginal route was that it caused less shock, no abdominal scar, and gave a lower general mortality. Since 1905 the latter objection to the abdominal route has been swept away, and we now have 15 cases of the Caesarean-Wertheim operation in which all the patients recovered and all the children were born alive.

It is only fair to look into the results obtained by the advocates of the vaginal route.

In 1906 Dührssen published a list of 46 cases in which vaginal Caesarean section followed by vaginal total extirpation was carried out. There were 5 deaths. This series did not include Dr. Wilson's 2 cases, and since then, Nagel, Seitz, Büttner, and Graefe have recorded 1 case each, making in all 52 cases with 5 deaths.

These results are exceedingly good, and compare favourably, as far as primary mortality of the mother is concerned, with the Caesarean-Wertheim method. What we still wait for is a comparison of after-results—the fate of our patients after a lapse of five years, and also a record of the child mortality. The data concerning the latter I cannot give in the cases of vaginal Caesarean section, but I agree with Dr. Munro Kerr in regarding the vaginal route as the more dangerous for the child.

Now that Wertheim's technique is becoming more popular in this country for cases of carcinoma of the cervix uncomplicated by pregnancy, I believe it will eventually prove to be the operation of choice for cancer complicating pregnancy also, especially when we remember that the lax state of the cellular tissues due to gestation is all in favour of the operator, and makes the procedure much more easy than it is found to be as a general rule. I have myself performed Wertheim's operation thirty-three times—that is to say, since my report at the meeting of this Association at Exeter in 1907 I have had 20 cases in which I have carried out Wertheim's technique for cervix carcinoma. Of these, I lost 3 cases—one from autoinfection from a pyometra, one from ileus without peritonitis, and one from diabetic coma fourteen days after operation; thus my mortality in my last 20 cases is 15 per cent. The Caesarean-Wertheim operation was the easiest performed of them all,

for reasons already given; in fact, had my other cases been so favourable, I believe the mortality would have been *nil*.

DISCUSSION.

Dr. MUNRO KERR (Glasgow) entirely agreed with Dr. Lockyer that the Wertheim operation was the best treatment. When the speaker recommended the combined abdominal and vaginal operation some years ago, in 4 cases of carcinoma of cervix complicating advanced pregnancy, he believed that at the time that was the best treatment. He was glad to learn that the Wertheim operation was really easier in the gravid than in the non-gravid condition.

Dr. S. CAMERON (Glasgow) stated that the gravid uterus could always be brought up easily into the abdominal wound. He was certain from the pathological condition present—namely, the early involvement of the lymphatics of the ureters—that within a short time all operators would divide the ureters and transplant the cut ends into the bladder. The wisdom of adopting this step was borne out by Leitch's researches into this disease. The speaker believed he had two specimens of involvement of the lymphatics of the ureter by a cancerous extension from the cervix on exhibition in the pathological museum in connexion with the Section.

Dr. CUTHBERT LOCKYER, in reply, said he was glad to find that both Dr. Wilson and Dr. Munro Kerr approved of the method he had employed for dealing with cancer of the cervix in the later months of pregnancy. He had endeavoured to make it quite clear in his paper that at the time Dr. Munro Kerr advocated Zweifel's combined method, and Dr. Wilson favoured vaginal Caesarean section, their data regarding the Wertheim operation was too scanty to draw any conclusions, or even to make a comparison between it and the vaginal method of operating. He was very gratified to hear Dr. Munro Kerr say that he believed the Wertheim operation would be the one of election after Caesarean section for operative carcinoma cervicis. He could not support Dr. Cameron's view that the ureters should be divided and subsequently implanted in the bladder as a routine practice. He had met with a case in the last few weeks where the last inch of the right ureter was embedded in cancer, which necessitated the resection of the ureter to that extent, but in such advanced cases it was very questionable if the patient derived much good from the operation. Dr. Munro Kerr had raised the question as to whether venous bleeding was a trouble in Wertheim's technique for pregnancy cases. It had not been so in Dr. Lockyer's case, and he attributed the fact to the powerful action of the large parametric forceps, and to care taken in transfixion of the connective tissues. In fact, the risk of haemorrhage was said (by those who had employed both methods) to be far less by the abdominal than by the vaginal route.

SECTION OF HAEMATOLOGY AND VACCINE THERAPY.

Sir ALMROTH WRIGHT, M.D., F.R.S., President.

THE INOCULATION TREATMENT OF TUBERCULOUS ARTHRITIS.

By MAYNARD SMITH, M.B., B.S.Lond., F.R.C.S.,
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THE revival of the administration of tuberculin during the past few years has been coincident with and dependent upon the work of our President on opsonins and the opsonic index, but this work has not alone been applied to the regulation of dosage as regards amount and timing, but has also been brought to bear upon the question of diagnosis and the problem as to when a cure may be considered to have been accomplished.

I have endeavoured to set down in this paper the results of investigations which I have carried out from the point of view of a surgeon into the value of tuberculin administration in the treatment of tuberculous joints, and the utility of the opsonic index in the diagnostic and therapeutic problems just mentioned. The patients who had been under treatment in the Inoculation Department of St. Mary's Hospital supplied the material for this investigation.

1. *The Problem of Diagnosis.*

I need not remind you that the effect of a dose of tuberculin is to produce a fall in the tuberculo-opsonic index (the negative phase—of greater or lesser degree, according to the dose), quickly followed by a rise up to and above the normal. This effect is produced either in a healthy or in a tuberculous individual. In a patient who has a focus of active tuberculous disease, constant autoinoculations are taking place from that focus, and each of these is followed, more or less regularly, by similar fluctuations of the opsonic index. It is clear, therefore, that in a tuberculous patient observations of the opsonic index to tubercle may show that index to be above or below the normal limits. This constitutes the first method of applying the opsonic index to the problem of diagnosis. There are, however, two fallacies associated with this method. In the first place, although the index of a tuberculous patient is at times above and below normal limits, yet for a considerable proportion of any period it is within those normal limits. Many examinations must therefore be made before a negative diagnosis can be justified. In the second place, it is quite possible for a patient to have a knee which is the seat of, say, congenital syphilitic disease and also a tuberculous focus in the thorax. The thoracic focus may be causing variations in the tuberculous index which, if this test alone be relied on, may be attributed to the joint lesion. For this reason, when a diagnosis is needed, other methods are employed. The principle of these methods depends on the production of an auto-inoculation of the patient from the lesion under examination and by subsequent estimation of his tuberculo-opsonic index, at intervals of a few hours, to ascertain whether that fluctuation had taken place which would have been expected had a tuberculous auto-inoculation been produced. The auto-inoculation is brought about either by massage or a Bier's bandage. In the case of tuberculous joints in the lower extremity, causing the patient to walk on the affected limb affords another useful method. [A table of cases illustrating the employment of auto-inoculation tests in diagnosis was passed round, and charts were shown.]

Before leaving this part of the subject I wish to add that the cases in the table printed herewith were, almost without exception, sent to the Inoculation Department of the hospital with a diagnosis of tubercle from a surgeon. A few were sent over with a doubtful diagnosis. In the former class inoculations were begun forthwith; in the latter, experiments were done with a view to decide the diagnosis. In one case which was sent over with a diagnosis of tuberculosis no diagnostic experiments were carried out; after six months no improvement had been made, and the patient ceased to attend. In response to a communication she came up with the other patients to be inspected. It occurred to me at once that it was a case of rheumatoid arthritis, and I asked that diagnostic experiments might be made. The results of these were negative, and the idea that the case was not of a tuberculous nature was confirmed.

2. *The Influence of Tuberculin Treatment.*

In dealing with the results of treatment I have a difficult task in front of me. I have, therefore, collected and represented as clearly as I could, in tabular form, the particulars and results of treatment in all cases which have been under treatment at St. Mary's Hospital by inoculation during the past few years, utilizing only those cases which I have been able personally to inspect in order to judge of their present condition. Of 72 cases written to, 34 attended; others wrote and regretted their inability to attend, but said that they were well and at work; others did not reply; others were in hospital elsewhere (see Table). Of these 34 that attended, I have attempted to lay before you sufficient information to enable you to formulate conclusions; the series is, however, a small one, and it shall be my endeavour to rectify this fault on a future occasion. There are, however, factors which cannot be put in tabular form, and these I would ask you to note.

TABLE OF CASES OF TUBERCULOUS ARTHRITIS TREATED BY VACCINES.

No.	Age and Sex.	Disease.	Date of Onset.	Duration and Nature of Inoculations.	Treatment Previous to Inoculation.	Additional Treatment during Inoculation.	Condition at Start of Inoculation.	Condition in July, 1909.
1	16, F.	Tuberculous shoulder (confirmed by indices of 1.46 and 0.58 at interval of four days)	June, 1906	Aug., 1906, to May, 1907. Tuberculin	*Nil*	Poroplastic shoulder cap	Pain in shoulder; limited mobility; wasting.	Shoulders normal; no difference on two sides.
2	13, M.	Tuberculous knee	June, 1906	Dec., 1906, to Nov., 1907. Tuberculin	*Nil*	Splint	Knee flexed; no movement possible; wasting; pain	Knees normal; no difference on two sides.
3	21, F.	Tuberculous dactylitis and hip	1892	Has had inoculations for two months only. Tuberculin	—	—	—	No change at present.
4	4, M.	Tuberculous disease of tarsus	October, 1907	Oct., 1908, until present time; still under treatment. Tuberculin	Plaster case	Plaster case	Ankle swollen; rigid; pain; mother describes child as "quite transparent."	No swelling; about 10 degrees of movement; general condition of child excellent.
5	38, F.	Tuberculous wrist (as a child had tuberculous elbow and dactylitis)	January, 1907	Feb., 1907, to Oct., 1907. Tuberculin	*Nil* for wrist	Carr's splint	Wrist swollen; painful; hot; impaired mobility	No swelling or pain; some deformity of wrist; movement through 20 degrees; uses wrist and hand for light work
6	16, M.	Tuberculous hip	1898	March, 1907, to Jan., 1908. Tuberculin	In hospital with extension; also as out-patient in splint	Splint	Great pain; wasting; ¾-in. shortening; trochanter above Nélaton's line; was getting worse	Walks fairly; occasional pain; head of bone deformed and dislocated; fair movement; very little wasting.
7	4, M.	Tuberculous disease of ankles and tarsus	January, 1907	July, 1907, up to the present time; still under treatment. Tuberculin and staphylococcic vaccine	Splints; three operations	Splints	Both ankles swollen; rigid; extended; two sinuses	Slight puffiness inner side of right ankle; otherwise normal; walks well; sinuses healed (healing was complete in July, 1908).
8	22, F.	Tuberculous knee	1901	July, 1907, up to present time; still under treatment. Tuberculin	Plaster and rest in bed; in hospital for a year	Splint	Knee swollen; painful; fixed; straight	Knee fixed in straight position; much less swelling and pain; walks, but pain and oedema of leg after much exercise.
9	5, F.	Tuberculous disease of ankle and tarsus	June, 1908	Nov. 1908, up to present time; still under treatment. Tuberculin and staphylococcic vaccine	Plaster	Plaster	Ankle and tarsus swollen; painful; inverted; rigid; sinus	Much improvement; sinus healed; some swelling over tarsus; still inversion and limitation of movement.
10	7, M.	Tuberculous disease of knee	July, 1907	Feb., 1909, up to present time; still under treatment. Tuberculin	Extension; Thomas's knee splint	Thomas's knee splint	Swelling; pain; flexion; limitation of mobility; not improving	No improvement up to present.
11	12, M.	Tuberculous knee	January, 1907	Sept., 1907, to Jan., 1909. Tuberculin	Thomas's knee splint	Thomas's knee splint	Swelling; pain; limitation of movement; no flexion	Knees normal; no difference on two sides.
12	7, M.	Tuberculous knee	July, 1906	Oct., 1906, to April, 1909	Extension; poroplastic splint	Splint	Swelling; pain; almost fixed in straight position	Flexion possible to 60 degrees; no pain or swelling; still wasting of limb; walks.
13	10, M.	Tubercle of head of tibia; afterwards tubercle of knee-joint; secondary staphylococcic injection with many abscesses and extensive bone infection	January, 1907	April, 1907, up to present time. Tuberculin and staphylococcic vaccine	Operation on head of tibia	Fourteen operations; chiefly opening abscesses from secondary infection; splint	Multiple sinuses and abscesses around knee and tibia; great swelling of knee and rigidity; hectic fever; general wasting; amputation considered	All healed; no pain; general condition good; movement free, and walks without pain.
14	21, M.	Tuberculous hip	1904	March, 1907, up to present time. Tuberculin	Thomas's splint for three years	Thomas's splint	Pain in hip and knee	Can find no evidence of active disease; movements somewhat limited; still complains of pain
15	6, M.	Tuberculous ankle and cervical glands	March, 1906	Aug., 1906, to February, 1907. Tuberculin	Plaster; operation	Plaster	Two sinuses; ankle swollen; rigid; "cries night and day"	Sinuses healed; runs and walks normally; no pain; "never complains" (condition in February, 1907).*
16	43, M.	Tuberculous knee	March, 1905	March, 1909, up to present time; still under treatment. Tuberculin	Splint; operation	Splint. Bier's bandage	Very swollen; painful; starting pains at night; rigid	Patient says it is "a treat." Some thickening of synovial membrane; movement through 10 degrees; no pain.
17	25, M.	Tuberculous knee (as a child spinal caries)	1893	June, 1906, up to present time. Tuberculin	Splint	Splint. Aspiration of pus July, 1908. Abscess in thigh opened and scraped September, 1908	Knee swollen; painful, doughy, rigid; could not walk	No pain; movement through 5 degrees; thickening of synovial membrane has entirely disappeared; can walk without pain.

* Since the above note was made, there has been a slight recrudescence of disease, the ankle becoming puffy and painful. This is rapidly subsiding with rest and tuberculin.

TABLE OF CASES OF TUBERCULOUS ARTHRITIS TREATED BY VACCINES *(continued)*.

No.	Age and Sex.	Disease.	Date of Onset.	Duration and Nature of Inoculations.	Treatment Previous to Inoculation.	Additional Treatment during Inoculation.	Condition at Start of Inoculation.	Condition in July, 1909.
18	30, F.	Tuberculous knee	August, 1908	December, 1908, up to present time. Tuberculin	Bier's bandage	Plaster	"Chronic synovitis of left knee;" much swollen; fainted with pain at night; only slight flexion possible	Knee appears normal to examination except for some thickening each side of ligamentum papillae. No fluid; movement free through 60 degrees; still complains of very great pain.
19	18, M.	Tuberculous disease of head of tibia and knee-joint	December, 1905	March, 1906, to July, 1909. Tuberculin and staphylococcic vaccine	Operation in February, 1906, head of tibia scraped out; splint	Second scraping; splint	Sinus from head of tibia; joint swollen; limitation of movement	Sinus healed; knee movement normal. Whilst inoculation was being carried out extensive ulceration of leg developed, which became gradually worse. These ulcers had the appearance of those due to syphilis, and the history of the case was suggestive of congenital syphilis. Wassermann reaction positive.
20	16, F.	Tuberculous knee	December, 1905	Sept., 1906, to Jan., 1907. Tuberculin	Thomas's knee splint	Thomas's knee splint	Right knee much swollen; limitation of mobility; pain on movement	Knee normal; no difference on two sides.
21	22, F.	Tuberculous knee	1904	June, 1906, to Sept., 1907. Tuberculin	Splint	Splint and Scott's dressing	Knee much swollen; limitation of mobility; wasting of thigh; flexion; not much pain	Knee normal; no difference on two sides.
22	51, F.	Tuberculous hip	Childhood. Symptoms recurred May, 1906	July, 1906, to July, 1907, and May, 1909, up to present time; still under treatment. Tuberculin	Several operations; bone removed (as a child)	Splint	Two sinuses; hip rigid and flexed	Sinuses closed and pain gone by July, 1907; then sinuses opened again May, 1909, with swelling and pain; inoculation recommenced; rapid improvement.
23	7, F.	Tuberculous knee, ankle, and hand (dactylitis)	October, 1907	Feb., 1908, to May, 1909. Tuberculin	Operations on hand and ankle (erasions of joint and scraping)	Splint	Swelling of synovial membrane; limitation of movement; wasting; no pain	All foci appear cured. Movements good.
24	$2\frac{10}{12}$, M.	Tuberculous ankle	January, 1908 (?)	Feb., 1908, to April, 1909. Tuberculin. Staphylococcic and streptococcic vaccines	Operation February, 1908 (erasion and scraping of bone)	Plaster	Three sinuses; ankle swollen; painful; stiff	Normal ankle; sound scars; walks with slight limp.
25	19, F.	Tuberculous elbow	March, 1904	March, 1906, to June, 1906 (then ceased to attend). Tuberculin	Splint for one year	Splint	Elbow swollen; painful; rigid	When inoculation discontinued, no change; arm now fixed at right angle; no pain.
26	5, F.	Tuberculous hip	December, 1906	May, 1908, to Feb., 1909 (then ceased to attend). Tuberculin	Two years in Thomas's splint; convalescent homes; during this time improved	Thomas's splint	Hip rigid and flexed; no pain except on attempted movement	Improvement continued in same way as before inoculation until abscess developed in Feb., 1909. Patient then admitted to hospital and operated on. Since then has not been inoculated. Now discharging sinus.
27	18, M.	Tuberculous hip	July, 1906	Oct., 1906, to June, 1907. Tuberculin	Rest; Thomas's splint; crutches	Splint and crutches	Pain; could not sleep; hip rigid; limb wasted	Hip normal.
28	$4\frac{9}{12}$, M.	Tuberculous knee	January, 1906	April, 1907, up to present time. Auto-inoculation test now negative, and treatment will be stopped. Tuberculin	Thomas's knee splint. Rest	Plaster	Swollen; painful; limitation of mobility	Walks well; no pain; flexion possible through 30 degrees.
29	40, F.	Tuberculous disease of both wrists and both ankles	1902—left wrist 1905—other joints	May, 1906, to June, 1909. Tuberculin	Operation, 1902, left wrist (erasion of wrist-joint); splints	Splints for wrist; bandages for ankles: did not lie up	Ankles swollen and painful; could not walk; wrists swollen and painful; fingers stiff and useless	Walks well, but left ankle weak; slight thickening about right wrist; uses hand and writes easily; no pain.
30	$3\frac{3}{12}$, F.	Tuberculous disease of left knee (confirmed by auto-inoculation experiment)	August, 1907	Sept., 1907, to Jan., 1908. Tuberculin	Splint (knee became worse)	Splint	Knee swollen; limb wasted; limitation of mobility	Knee normal; no difference on two sides.
31	58, M.	"Compound palmar ganglion" and tuberculous disease of carpus	November, 1906	July, 1908, to Jan., 1909. Tuberculin (then ceased to attend)	Splint. Operation; scraping out palmar synovial sac; (melon-seed bodies); disease of carpal bones	Splint; Bier's bandage	Sinus; wrist stiff; fingers stiff; great pain	Wrist moves well; no pain; sinus; slight limitation of movement of fingers.
32	18, F.	Tuberculous disease of wrist (confirmed by index of 0.64)	September, 1906	Nov., 1906, to March, 1907. Tuberculin	Splint for six weeks; no better	Splint	Wrist swollen; almost complete rigidity; movements of fingers almost lost	Wrist normal; all movements of wrist and fingers good; general health greatly improved.
33	12, F.	Tuberculous hip	1902 (wore Thomas's splint for one year then ran about normally) 1905 (symptoms recommenced; Thomas's splint again)	March, 1906, to July, 1907. Tuberculin	Thomas's splint; said to have been getting worse all the time	Splint until January, 1907; then started walking	Pain; rigidity; wasting; progressive shortening (noted as 2 in.)	Shortening 1½ in.; rigid; walks fairly well.
34	5, F.	Tuberculous right knee	May, 1905	Aug., 1905, to June, 1908. Tuberculin	Splint; said to have been getting worse all the time	Leather splint	Knee swollen; wasting; pain; movements of knee good	Knee normal; no difference on two sides. General health improved.

In the first place it will be noted that of the 34 cases only 3 (Nos. 1, 2, and 5) were treated by inoculation from the time that they first came under observation. Of these 2 were cured, with complete restoration of function; the third was cured, but some deformity and limitation of movement remained.

In 16 cases treatment by splinting, rest, and in some by operation, had been carried out for a period of twelve months or less before the administration of tuberculin was begun, and in the majority of cases it is but fair to state that it was because of the unsatisfactory progress of the case that inoculation treatment was instituted. In 10 of these 16 cases cure ensued with complete and absolute restoration of function (Nos. 11, 13, 15, 19, 20, 23, 27, 30, 32, 34); two (Nos. 12, 24) were cured with slight limitation of movement; four (Nos. 4, 7, 9, 18), are improved and still under treatment.

Of the remaining 15 cases which had been of more prolonged duration, 2 only had two or three months' treatment (Nos. 3 and 25); in two no improvement had taken place after six and ten months' treatment respectively (Nos. 10, 26); six are cured (Nos. 6, 17, 21, 28, 29, 33), and five are improved and remain under treatment (Nos. 8, 14, 16, 22, 31).

Eight cases had sinuses, and in all the sinuses healed under inoculation treatment except in the case of a man who ceased to attend after six months' treatment.

Lastly, it must be noted that these were out-patients; they had to visit hospital, as a rule, once in ten days; complete rest was therefore often impossible, and, moreover, such advantages as seaside convalescent homes were not available whilst they were under treatment.

I would touch for a moment on the "surgical" aspect of the treatment of these joints. It must be remembered that it is not sufficient merely to arrest the disease. It is essential, in addition to this, that the best functional result shall be obtained when the cure of the disease is complete. The province of the surgeon here lies in the direction of splinting, extension, and other similar methods; at a later date tenotomies and osteotomies may be called for. In the treatment of abscesses, the removal of masses of inert dead material, and the treatment of sequestra of bone, operation is necessary, and, in passing, I may say that aspiration of abscesses is the ideal method of treatment, although more radical measures in the way of opening and scraping may become necessary. Lastly, there remain a certain small proportion of cases where everything has failed, and amputation gives the best chance to the patient.

3. *The Question as to When a Cure has been Accomplished.*

I wish next to speak of the question of cure. It has always been a difficult question for the surgeon to decide when he might consider his case to be cured, when it would be safe for the patient to discontinue rest, to commence again to use the affected joint, to walk on the affected limb, and to discard the apparatus with which rest to the diseased part has been obtained. Most surgeons have some such rules as these: First, a minimum period of rest is usually insisted on, varying with the predilections of the surgeon; secondly, there must be no local pain, tenderness or heat of the affected joint; thirdly, there must be no effusion into the joint, nor must there be any great swelling or thickening of the synovial membrane; and, lastly, there should be no recrudescence of pain, etc., when use of the joint is again permitted. These rules are excellent, and have served their purpose well. Nevertheless, there must be few surgeons who cannot call to mind cases in which a rapid and disastrous recrudescence of disease has followed cessation of rest and splinting, when all clinical evidence of the disease had completely disappeared. Again, more than one surgeon having determined not to err on the side of boldness, has ordered "just another three months in splints," to find that the anxious and tired parents have taken the child to a bonesetter, who has replaced a small bone in the hip joint, hurled the splints away, and, with the orthodox dramatic gesture, bidden the child to arise and walk. No ill results have followed, and the bonesetter has been firmly installed in the minds of parents, relations, and friends, as the man who cured in ten minutes a hip which Mr. X. had been attending for two years without success. Are we not, then, prepared to admit that there is a place for some method which will aid us in determining when a cure has been arrived at? It is claimed that experiments in ordinary inoculation similar in every way to those employed in diagnosis will supply this aid. [Tables were passed round and charts were shown illustrating the employment of auto-inoculation tests, with a view to decide whether the disease was cured.]

THE TREATMENT OF BRONCHIAL ASTHMA BY A VACCINE.

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Many persons who suffer from chronic bronchitis complain of attacks of spasmodic dyspnoea as an especially distressing symptom. These attacks are generally called "bronchial asthma." They are precipitated by any exertion, and are also prone to occur at night and disturb sleep, the patients very frequently complain that they cannot lie flat, but have to be propped up in bed. Spasmodic dyspnoea occurs, further, in heart disease and uraemia, and also as a more or less independent affection, sometimes called "true" asthma; how far the latter is independent is a matter of dispute. The symptom seems to be the same thing in whatever disease it occurs, and it may be regarded as a reflex disturbance which is precipitated by a variety of causes.

The object of this paper is to suggest that one cause of spasmodic dyspnoea in chronic bronchitis is a specific bacterial toxin, the result of a definite infection and amenable to treatment by the corresponding vaccine.

In 1907, while making some investigations into the bacteriology of chronic bronchitis, in the laboratory of the Inoculation Department at St. Mary's Hospital, I isolated a certain organism in nearly pure culture from the sputum of a female patient. I took her opsonic index to this, and finding it low, I suggested inoculation, to which she agreed. She was given a dose of 25 millions hypodermically, and was instructed to come back in two days. She suffered severely from bronchial asthma. On her return she said that though her cough was no better her breathing had been much relieved. The notes of her case are as follows:

Case I.

L. L., aged 29. Chronic bronchitis and asthma since age of 16. Father died of same.

November 23rd. Inoculated 25 millions. Felt better the same evening.

November 24th. Marked improvement in breathing, able to light the fire for the first time for seven weeks.

November 25th. Not so well, but better than usual.

December 2nd. Continued improvement; one slight attack of dyspnoea after a heavy supper. Cough better. Inoculated 30 millions.

She was inoculated weekly throughout December without any recurrence of dyspnoea, and after that did not attend till March 4th, when she had the first recurrence and was inoculated again; she attended throughout March and April, and had in that time two slight attacks. After that date she did not attend again.

I had at this time access to a large clinic as Casualty Physician at St. Mary's Hospital, and I used the same vaccine extensively among patients suffering from bronchial asthma, in about 70 cases in all, and of these I have collected 52, who gave the experiment a fair trial, that is, who attended for inoculation at least twice. The degrees of disability at the outset and improvement under treatment were very various, so that any tabular presentation of the results would be arbitrary and misleading. The results obtained will be best indicated by quoting a few cases at length:

Case II.

E. B., aged 24, carman. Asthma since age of 6, worse in winter, especially in rain and fog. Attacks at night and when at exercise.

January 6th, 1908. Inoculated 50 million.

January 9th. Breathing much better.

January 10th. Breathing very good since attending, but cough troublesome. Able to go up and down stairs without difficulty. Inoculated 50 million.

January 30th. Breathing still good; much better than for nine months previous; quite normal for three weeks. This is the first time he has been through any part of the winter in London without asthma. Inoculated 50 million.

April 2nd (two months later). Kept well till three days ago; now slight recurrence. Has lost no work; has slept well. No definite attacks, but trifling dyspnoea on rising in the morning and after meals. Inoculated 50 million.

June 3rd (two months later). Slight recurrence. Has been able to run with ease for the first time in his life, has run as much as three quarters of a mile. Is playing cricket, has never been able to play any game for more than ten minutes before. The patient volunteered this information. Six weeks later he had a recurrence, but owing to my own absence from London I lost sight of him.

CASE III.

G. C., aged 24, employed in motor garage. Chronic bronchitis and asthma for several years. This patient has kyphosis and a deformed chest, and very marked physical signs of chronic bronchitis.

May 6th, 1908. Inoculated 50 million.

May 13th. Slight improvement in sleep and breathing. Inoculated 100 million.

May 27th. Short of breath when at work, otherwise better. Sleeps better, sometimes sleeps night through. Inoculated 100 million.

June 3rd. Much better, sleeps night through. Less short of breath on exertion, has run for the first time for a long time. Is better than he has been for four years. Inoculated 100 million.

June 7th. Slight recurrence. Now carries water and pushes motors. Inoculated 100 million.

July 15th. Away a month; recurrence in change of weather. Inoculated 75 million. Did not attend again.

CASE IV.

M. L., 25, domestic servant, had suffered for two years from attacks of dyspnoea, which occurred regularly once a fortnight, and lasted for three days, occurring at night and forcing her to sit up in bed. She was well in the intervals. She was inoculated every week from July 23rd, 1908, to September 9th. The attacks occurred for the first two fortnights as before, but only lasted two days instead of three, and were less severe than before. On the third occasion no attack occurred till the third week had begun, when she had abortive attacks on two days and a severe one on the third.

After September 9th she did not attend till November 18th, during which time she was free from attacks for seven weeks, after which they began again at fortnightly intervals, but with less severity. She was inoculated again at intervals, till March, 1909, and during this period had only three slight attacks.

CASE V.

G. M., signalman. Chronic bronchitis and asthma for two years. Attacks occur chiefly at night and on exertion.

February 10th, 1908. Inoculated 40 million. On February 12th he wrote: "My breath so far has been excellent. I have had two much better nights' rest than I have had for a long time."

February 19th. Very marked improvement indeed. Sleeps well. Came upstairs, a flight of about twenty steps, without obvious dyspnoea. Far less appearance of distress. Inoculated 40 million.

March 3rd. Has not missed an hour's work. Previous best four and a quarter days per week. One attack of sneezing associated with dyspnoea.

March 20th. Improvement maintained till this date, when he had severe recurrence for three days. Inoculation was continued till April 22nd without recurrence, after which he kept well for four months without further treatment till August, and gained 8 lb. in weight. He then had several bad attacks for three weeks. He attended for inoculation about once a month till the following March with two slight attacks and one severe one lasting a fortnight. He was accustomed to use two stramonium powders every night, and of late has reduced them to one a week.

These cases are selected ones and represent the best results obtained, the average is not as good as these, but at an arbitrary estimate striking benefit has been obtained thirteen times, or in about one-fourth of the cases.

Taking results as a whole, 31 cases have found some degree of improvement in the frequency and 39 in the severity of their attacks: 26 have improved in their powers of taking exercise and 29 have slept better. In some cases improvement has been slight and in others temporary. In 4 cases no improvement at all has resulted.

In all these cases I have avoided, as far as possible, the use of the usual remedies for asthma. This asthma is a symptom which is liable to great spontaneous variation, and further, it is largely a subjective symptom, so that the records of results depend very much on the patient's statements, not usually considered a source of unpolluted veracity, but even with these allowances, when in three instances out of four in a total of 50 cases, patients inoculated with a specific vaccine find subjective improvement of the same symptom, that may be considered presumptive evidence that the symptom is due to infection with the corresponding organism.

The search for this organism in other cases has been most disappointing. I have examined the sputum of at least 50 cases, and I have only succeeded in finding it in one other instance, and from this specimen an efficient vaccine was made.

Attempts at serum diagnosis have been equally unsatisfactory. Although in the first case I found a low opsonic index I was not able, as a rule, to obtain any valuable information by this means.

There was a very wide variation between normal persons, and a series of daily observations on certain patients who were improving and on others who were not revealed no correlation between the index and the clinical condition. Further, I have been unable to obtain agglutination of the organism with the patients' serum.

The characters of the organism are the following:

It is a short bacillus with rounded ends, about the size of a pair of pneumococci. The size varies, however; short forms are found which appear like cocci, and occasionally long forms occur. It stains readily with basic aniline dyes, and is decolorized by Gram's method. There is marked polar staining, the unstained portion in the middle being so clear as to suggest that one is looking at a pair of diplococci. In an opsonic preparation, however, organisms which have undergone phagocytosis stain homogeneously and are seen to be bacillary in form. It has no visible capsule, is non-motile, and does not form spores. The bacillus grows readily on agar and gelatine and in broth; its growth is not inhibited by the presence of bile salts. On agar the colonies are pale grey, circular, and slightly raised. They vary in size, but as a rule are smaller than staphylococcus and larger than streptococcus; in a stroke culture they form a continuous line along the track of the needle, with scattered colonies at its edge. The growth is soft, but less wet than Friedländer's bacillus; it is not tenacious, and is readily emulsified. It grows best at 37° C., at which temperature there is a visible growth in six hours and a profuse one in twenty-four hours. A visible growth occurs in twenty-four hours at 15° C.

It does not liquefy gelatine, it does not coagulate milk, and it does not form acid or gas in any of the following sugars: Cane sugar, glucose, lactose, dulcite or mannite. I have been unable to identify this organism with any described in the textbooks. In appearance and growth it is similar to Friedländer's bacillus, and I have several times isolated bacilli probably belonging to the same group, but in only the two cases mentioned have I found an organism which had no power of fermenting sugars.

Dr. Freeman kindly inoculated a rabbit, a guinea-pig, and a mouse for me, but the animals were all apparently unaffected.

That an organism should be so easily cultivated and yet so rarely found is enough to excite scepticism as to its causal relation to any disease, and it certainly falls far short of fulfilling Koch's postulates. I have found that patients suffering from bronchial asthma frequently obtain benefit from the use of this vaccine, and suggest the probability that this organism is a cause of spasmodic dyspnoea. I have not succeeded in proving this, but it is possible that with research the proof may be forthcoming. In conclusion, I wish to offer my thanks to Sir Almroth Wright for the use of his laboratory and for his sanction; and also to Dr. Willcox for the greatest encouragement and for facilities for making observations on patients under his care in the wards of St. Mary's Hospital.

THE TREATMENT OF PNEUMONIA BY INOCULATION.

BY

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and

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IN this paper we give the results of treatment by inoculation of 24 cases of pneumonia. These were cases either of lobar pneumonia or of primary lobular pneumonia (primary bronchopneumonia). In all of the cases the organism present in the lung was the pneumococcus. In two of the cases the pneumococcus was combined with a streptococcus and the influenza bacillus respectively. It is well known that pneumonia is a disease having a high case incidence. Probably in England and Wales the average number of cases is over 100,000 per annum, and the average mortality is about 40,000 per annum. Pneumonia attacks persons of all ages, and it occurs at all seasons of the year.

The examination of statistics shows a close relationship between influenza epidemics and pneumonia, the incidence and mortality of the latter being much higher during the prevalence of influenza. Also, there is no doubt that pneumonia is infectious, and this may explain the frequency of several cases of the disease occurring in the same household or in the same locality. The severity of attacks of pneumonia may show great variation owing to the variation in the virulence of the infecting organism.

Place of Vaccine Treatment.

Since pneumonia is one of the most important diseases, and, as illustrated above, since it accounts for the loss of a great many valuable lives which are snatched away with great suddenness, it is obvious that any therapeutic measure which can increase the resistance of the patient to the disease will be of great value and will be the cause of saving many lives. We have used in all our cases a bacterial vaccine, in addition to the ordinary approved therapeutic measures. Most of the cases have been patients in St. Mary's Hospital, London, under the care of one of us, and the inoculation work has been carried out in the laboratories of Sir Almroth Wright, to whom we are indebted for advice and assistance throughout the work.

It is important to remember that inoculation is in no way intended to replace the usual methods of treatment; thus, where necessary, leeches, sponging, icebags, cardiac tonics, oxygen, etc., have been used. We believe the administration of an appropriate vaccine to be a very valuable therapeutic measure, and that it should be used in addition to the usual methods of treatment. In no single case have we seen the slightest harm result from inoculation. In many cases the benefit following the vaccine has been very striking and marked.

Statistics.

We do not propose to give any statistics based on our cases, for though our results have been extremely good, yet we feel that the number of cases is too small for the expression of a mortality-rate of cases treated by vaccine. Statistics as to the rate of mortality in pneumonia must always be misleading since the disease varies so much. The severity of any attack will depend on the nature and virulence of the bacterial infection, also on the age, physical condition, and habits of the patient. Each individual case must be judged on its merits.

Methods of Procedure.

It is obvious in order that vaccine therapy may be effective that the nature of the infecting organism or organisms must be known. In the large majority of cases the pneumococcus is the causal organism, but sometimes the disease is due to an organism other than the pneumococcus. In some cases—for example, Case I and Case XVI of our series—the pneumococcus is combined with another organism. There are three procedures available, and they depend respectively on—

1. Examination of the sputum.
2. Blood culture.
3. Aspiration of the pleural cavity or superficial part of the lung.

Since pneumonia is a disease which runs a short course it is most desirable that vaccine treatment should be commenced early and that as little time as possible should be lost. The majority of the cases of acute pneumonia are due to a pneumococcal infection, and those cases which are due to other organisms usually present clinical symptoms and signs which indicate the nature of the causal organism. Hence it is advisable that in an ordinary case of pneumonia presenting signs and symptoms of a pneumococcal infection a dose of stock pneumococcus vaccine should be immediately administered. At the same time steps should at once be taken to prepare an autogenous vaccine as rapidly as possible.

The three methods mentioned are as follows:

Method 1.—The sputum is carefully examined and cultures are made on serum or blood agar. It is usually necessary to make subcultures in order to obtain a pure pneumococcal growth. This of course involves delay, so that as a rule 36 to 48 hours elapse before the autogenous vaccine is ready.

Method 2.—A blood culture may be made as follows: About 10 c.cm. of blood are taken from one of the veins of the arm with the usual antiseptic precautions. The blood is added in equal parts to four tubes of ordinary broth, and these are incubated. Sometimes after 12 hours a good growth may be found, but at other times the clot encloses the organisms, which have to be shaken out, and it may then be necessary to make subcultures in order to have sufficient growth to prepare a vaccine, which of course involves delay. Frequently the tubes of broth after admixture with the blood, and incubation, remain sterile.

Method 3.—The third method, which we have much used in our cases, is that of puncture of the chest wall in the region where the signs of consolidation are most marked. A 1 c.cm. syringe with a fine needle is used. About ½ c.cm. of sterile broth is aspirated into the syringe and the needle is passed into the pleural cavity or surface of the consolidated lung. A small quantity of the contained broth is then injected and immediately aspiration is performed by withdrawal of the piston. The needle is now gradually drawn out of the chest, the position of the piston being such that there is a negative pressure within the syringe. In this manner a mixture of blood plasma and broth is obtained, which is added to two culture tubes containing sterile broth. This forms a good medium for the growth of the organisms. The tubes are now incubated and occasionally shaken. Within 12 hours it is often possible to obtain a growth containing 1,000 million per cubic centimetre. The contents of the tubes are then sterilized at 60° C. for one hour and are diluted with ½ per cent. carbolic acid solution to a suitable strength, such as 100 million per cubic centimetre. By this procedure it is sometimes possible to inoculate with an autogenous vaccine in 12 hours. It should be added that not infrequently the fluid withdrawn from the chest by aspiration as described above will prove sterile.

With regard to the three methods above described, the following remarks may be made:

Method 1 causes no discomfort to the patient. It is, however, inapplicable to children, owing to the absence of sputum. Sometimes the organism obtained by cultures from the sputum, although a diplococcus, has different characteristics from that obtained by methods 2 and 3, and may possibly not be the causal organism. This method, even under the most favourable conditions, involves considerable delay in the preparation of the autogenous vaccine.

Method 2 is a good method, but sometimes it involves delay, and in the case of children there may be difficulty in finding a suitable vein.

Method 3, involving puncture of the chest wall, has the great advantage of being the most rapid way of preparing an autogenous vaccine.

Although in our hands there have been no harmful results from the application of this method, yet it is possible that some haemorrhage might result if a vessel were punctured; this, however, is very unlikely if a fine needle is carefully used. It is also possible that some air might be introduced into the pleural cavity if healthy lung were pierced, but if great care be taken so that the part of the chest pierced is over consolidated lung, then it is extremely unlikely that any accidents of this kind should happen. The method is one which some patients might object to, since a little pain is caused. One advantage of this method of procedure is that if the pneumonia is due to an organism other than the pneumococcus, then one readily discovers this organism, and very important information is gained from the point of view not only of treatment, but also of prognosis.

Dosage.—After the preparation of the vaccine the dose to be administered has to be decided upon. At present there is some difficulty as regards guidance in determining the appropriate dose. In many infections the appropriate dose of vaccine is indicated by the opsonic index, but in pneumonia certain difficulties arise. Sometimes the pneumococcus obtained was not susceptible to the action of normal serum, there being no phagocytosis under any circumstances, and then no opsonic index could be obtained. In such a case one has to rely on indices obtained by using a suitable stock culture. Another difficulty is that of agglutination of the organism. This feature is seen late in the disease, especially in cases the course of which is protracted, and it causes great difficulty in the determination of reliable opsonic indices. We have found that sometimes the opsonic power of apparently healthy individuals to the pneumococcus obtained varies considerably, and thus there is difficulty in obtaining a reliable standard. This difficulty may be overcome by using for the standard a mixture of several serums from healthy individuals or else by using in the same series of experiments serums taken always from the same individuals.

We have found that near the crisis there is usually a definite rise in the opsonic power, and in some of our cases the rise has been most striking. In the present series of cases we have been unable to regularly use the opsonic index as our guide to dosage on account of the difficulties described. We have taken as our guide the clinical symptoms of the patient. We have found that an inoculation of from 20 to 50 million of pneumococci has never had any bad effect on an adult case, and we consider that a suitable initial dose would be from 20 to 30 million.

After twenty-four to forty-eight hours a second dose may be given, and further doses given at suitable intervals if necessary. We think that as the crisis is neared the dose should be decreased, because in the later stage of the disease the patient becomes more susceptible to inoculation, and there might be risk if too large a dose were given at this period. The dose of vaccine for children should be proportionately less than that given to adults.

CASES.

The following are the details of the 24 cases which have been treated by inoculation:

CASE I.

E. V., No. 2,164, admitted December 31st, 1907, female, aged 19. Onset December 22nd, 1907, vomiting, severe abdominal pain; these symptoms continued with increased severity. On admission general peritonitis was diagnosed and laparotomy performed by Mr. V. W. Low. No perforating lesion was found, and the peritonitis was considered to be due to a pneumococcal infection. From the fluid pure cultures of a pneumococcus and streptococcus were obtained by Mr. A. Fleming. After the operation patient continued very ill with much pyrexia, pain, and dyspnoea. On examination patches of consolidation were found in both lungs. From January 1st, 1908, until February 8th very acute symptoms continued with frequent rigors. Physical examination showed fresh patches of consolidation occurring continuously during this period.

Vaccine Treatment.—January 1st, 1908, 2½ million of stock pneumococcus vaccine; gave no benefit. January 5th, 1908, 5 million own pneumococci and streptococci each; gave marked benefit. January 23rd, 1908, 5 million own pneumococci and streptococci each; gave marked benefit. January 27th, 1908, 5 million own pneumococci; no benefit. January 30th, 1908, 10 million own pneumococci; no benefit. February 6th, 1908, 10 million own pneumococci and streptococci each; gave very marked benefit. February 13th, 1908, patient had a normal temperature, and was practically better.

This case was undoubtedly much benefited by vaccine treatment, and we think that the life was saved by it, since the symptoms were very severe and the ordinary therapeutic measures produced no effect on the symptoms. The great interest of the case lies in the fact that the infection was a double one, and that it was only with a combined vaccine that marked benefit resulted.

CASE II.

D. L., aged 28, male, admitted April 21st, 1908, No. 615. Onset acute, rigor April 19th, 1908. Clinical examination showed consolidation of lower and upper lobes of left lung and consolidation of the base of right lung. On April 23rd the crisis occurred and after this all symptoms abated.

Vaccine Treatment.—April 21st, 12 million stock vaccine. April 23rd, 25 million own vaccine. Crisis late on fourth day.

CASE III.

C. R., aged 38, male, No. 694. Onset April 2nd, 1908, severe pain in right side, cough, dyspnoea. Admitted April 5th, 1908, with consolidation of right lower lobe and of lower part of left lower lobe. Crisis occurred on April 10th, eighth day.

Vaccine Treatment—April 6th, 20 million stock vaccine. April 7th, 10 million own vaccine. Patient made a complete recovery.

CASE IV.

A. B., aged 14, male, No. 1,172, admitted August 7th, 1908. Onset August 4th, rigor, fever, severe pain in side, cough, dyspnoea. On admission there were all the clinical symptoms of pneumonia of a severe type, there was marked delirium, hallucinations of vision were present. There was marked dilatation of the left ventricle. There were definite signs of extensive consolidation of right lower lobe, and of some consolidation of left lower lobe at base.

Vaccine Treatment—On August 9th patient was given 2½ million of his own pneumococci, and in a few hours the crisis occurred (fifth day), and the patient was quite free from symptoms.

At the time that this case was under treatment a private case of almost exactly similar type in a boy was treated without vaccine. This latter case ran a much more severe course and almost succumbed.

CASE V.

M. T., No. 1,352, aged 3¾, female. Onset September 11th, feverish, sick, cough, dyspnoea. Admitted September 14th, 1908. Patches of consolidation found in both lower lobes. The symptoms were of a very severe type. The temperature fell by lysis, and on September 24th (the thirteenth day) became normal, and the patient was better.

Vaccine Treatment.—September 15th, ½ million stock vaccine. September 16th, ¾ million stock vaccine. September 19th, 1 million own vaccine. September 20th, 1 million own vaccine.

CASE VI.

I. B., No. 1,403, male, aged 2½. Onset September 19th, 1908, feverish, delirious, pain in chest. Admitted September 23rd, 1908. Extensive patches of consolidation in right lower lobe, and also of a less extent in left lower lobe. The symptoms were very acute.

Vaccine Treatment.—On September 24th was given 1 million stock pneumococci, and on September 26th (seventh day) the temperature fell to normal and symptoms abated.

CASE VII.

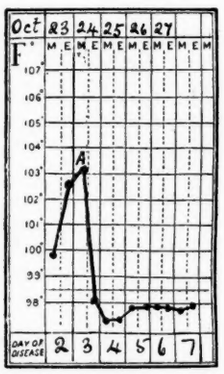
C. D., No. 1,432, male, aged 2¾. Onset September 29th, 1908 (the day of admission), sick, dyspnoea, cough, fever. On examination patches of consolidation were found in both lower lobes.

Vaccine Treatment.—September 30th, 1908, 2 million stock pneumococci. On October 1st (third day) the temperature fell to normal and the symptoms subsided.

CASE VIII.

E. F., No. 1,559, male, aged 6. Admitted October 23rd, 1908. Onset October 22nd, 1908, feverish, cough, sick, pain in left side, dyspnoea. On examination patches of consolidation were found over both lungs, more marked on right side.

Vaccine Treatment.—On October 23rd, 1908, 2 million stock pneumococci were given. On October 24th (second day) the temperature fell by lysis and all symptoms abated.



CASE VIII.—A. Two million stock vaccine.

CASE IX.

Mrs. M., aged 55. Subject to attacks of asthma for years. On November 1st, 1908, had fever, shivering, pain in left side, dyspnoea. There were signs of consolidation of left lower lobe. The symptoms did not clear up under the usual therapeutic measures. A vaccine was made with pneumococci from sputum.

Vaccine Treatment.—November 26th, 1908, 7 million own pneumococci. December 5th, 1908, 10 million own pneumococci. December 15th, 1908, 10 million own pneumococci.

After each dose of vaccine there was a marked improvement, and after the last dose the symptoms completely cleared up, so that patient was able to make the journey to a warmer climate. The case was an interesting example of the marked benefit which an appropriate vaccine has on a chronic pneumonia.

CASE X.

B. J., aged 40, male, No. 45. Onset January 5th, 1909, rigor, cough, pain in side. Admitted January 8th with all the clinical symptoms of pneumonia. On examination definite signs of consolidation of both lower lobes. Marked dilatation of left ventricle was present.

Vaccine Treatment.—January 10th, 5 million own pneumococci. January 11th, 4 million own pneumococci. January 14th, 7 million own pneumococci.

On January 14th, ninth day, the temperature reached normal, having fallen by lysis, and the patient made a complete recovery. The case was a very severe one.

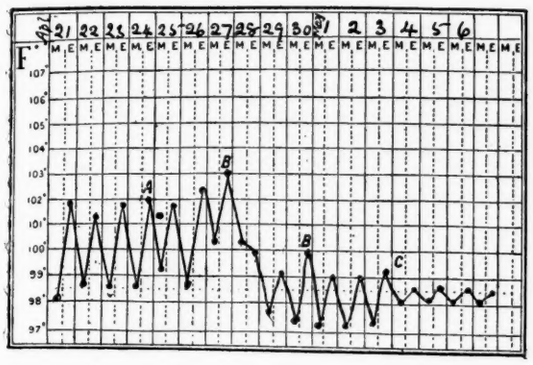
CASE XI.

R. S., No. 381, aged 14. Under Mr. V. W. Low. Operated on February 27th, 1909, for appendicitis, and abscess drained. The temperature remained high for many weeks after the operation. There were no signs or symptoms pointing to an abdominal cause. There was marked cough, dyspnoea, pain in side, and on examination there were signs of consolidation of both lower lobes. On numerous occasions explorations were made with an aspirating needle, but no pus was found in chest. The symptoms did not improve at all under ordinary treatment.

Vaccine Treatment.—April 24th, 5 million stock vaccine given with no benefit. April 27th, 10 million own pneumococci given with very marked benefit. April 30th, 10 million own pneumococci given, with result that all symptoms abated. May 4th, 12 million own pneumococci.

This is one of our most convincing cases, the patient had responded to no form of treatment until vaccine was given, and then it was only the vaccine made from the

patient's own pneumococci which was of benefit. When that was given the cure was almost instantaneous.



Case XI.—A, Five million stock vaccine; B, ten million own pneumococci; C, twelve million own pneumococci.

CASE XII.

L. H., No. 248, aged 9, female, admitted February 23rd, 1909. Onset February 21st, feverish, delirious, vomiting, cough. On admission patches of consolidation diagnosed in both lower lobes.

Vaccine Treatment.—February 24th, 3 million own pneumococci. February 26th, 4 million own pneumococci. February 27th, 5 million own pneumococci.

The patient had acute symptoms, which subsided by crisis on February 28th, the seventh day.

CASE XIII.

I. M., aged 3½, No. 249. Onset February 10th, 1909, admitted February 23rd. Definite signs of consolidation of patches of lung in both lower lobes. All the symptoms of acute bronchopneumonia were present. On February 26th, in consequence of severe meningeal symptoms, a lumbar puncture was done; the fluid was sterile.

Vaccine Treatment —February 26th, 6 million stock vaccine. February 28th, 6 million stock vaccine. March 2nd, 6 million stock vaccine.

The temperature fell by crisis on March 3rd, the twenty-first day, and the patient made a complete recovery. The case was one of great severity, and showed marked benefit from vaccine.

CASE XIV.

W. D., aged 60, No. 237, admitted March 2nd, 1909. Onset February 27th, rigor, pain in left side, cough. On admission patient was extremely ill, with definite signs of consolidation of left lower lobe. There was albumen in the urine.

Vaccine Treatment.—March 3rd, 20 million stock vaccine. March 4th, 20 million stock vaccine. March 6th, 15 million stock vaccine. March 10th, 16 million stock vaccine.

The temperature fell to normal on March 13th by lysis, the fourteenth day, and the symptoms of pneumonia subsided. The patient went on well until March 28th, when symptoms of nephritis developed, and he died from suppression of the urine. There was no pyrexia after March 13th, when the pneumonia subsided.

CASE XV.

J. P., aged 29, No. 291, male. Onset February 28th, with rigor; admitted March 2nd, 1909. On admission there were definite physical signs of consolidation of the left lower lobe and part of the right lower lobe.

Vaccine Treatment.—March 3rd, 20 million stock vaccine. March 5th, 20 million own pneumococci. March 6th, 20 million own pneumococci.

The temperature fell after each dose of vaccine, but more so after the autogenous vaccine; the temperature fell by lysis, reaching normal March 9th, and the patient made a complete recovery.

CASE XVI.

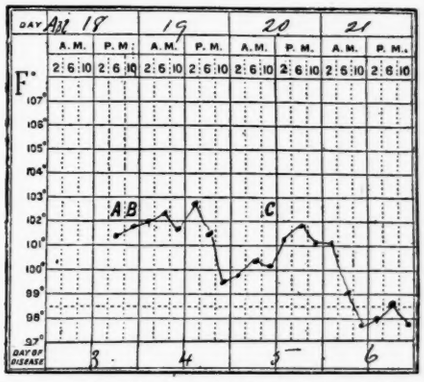
E. L., aged 6. No. 410. Admitted March 23rd, 1909. Patient was admitted with symptoms of pneumonia, which had been present for two weeks. On examination there was definite evidence of patches of consolidation at left base and in left axilla, also in the upper lobe of left lung. The symptoms continued in spite of all treatment, and there was pyrexia from March 23rd until May 26th, with cough, dyspnoea, and cyanosis, and the signs of fresh patches of consolidation of lung from time to time.

Vaccine Treatment.—April 18th, 6 million own pneumococci. April 22nd, 8 million own pneumococci. April 24th, 7 million own pneumococci. April 26th, 10 million own pneumococci and 8 million influenza bacilli. April 29th, 10 million own pneumococci and 10 million influenza bacilli. May 5th, 10 million own pneumococci and 10 million influenza bacilli. May 8th, 10 million own pneumococci and 10 million influenza bacilli. May 12th, 12 million own pneumococci and 10 million influenza bacilli. May 16th, 15 million own pneumococci and 12 million influenza bacilli. May 21st, 15 million own pneumococci. May 25th, 20 million own pneumococci.

This was a very striking example of a persistent pneumococcal infection. There appeared to be definite benefit from the vaccine treatment, since before this the case appeared quite hopeless.

CASE XVII.

A. S., No. 543, aged 18. Onset April 15th, 1909. Admitted April 18th. There were definite signs of consolidation of left upper and lower lobes, and of the base of the right lower lobe.



Case XVII.—A, Admission; B, thirty million stock vaccine; C, twenty million own pneumococci.

Vaccine Treatment.—April 18th, 30 million stock pneumococci. April 20th, 20 million stock pneumococci. Temperature fell by crisis April 20th, the fifth day, and all symptoms cleared up.

CASE XVIII.

J. B., aged 29. No. 597. Admitted April 27th, 1909. Onset April 25th, with rigor. On admission there were definite signs of consolidation of the middle and lower lobes of right lung, and of the lower part of left lower lobe. There was marked delirium.

Vaccine Treatment.—April 27th, 50 million stock, vaccine. April 30th, 20 million stock.

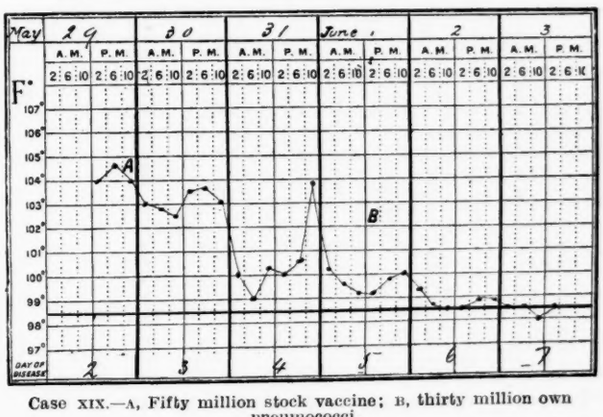
The temperature reached normal on May 2nd, the eighth day, having fallen by lysis. The symptoms showed definite improvement after the vaccine.

CASE XIX.

Mr. F. Onset May 28th, 1909. Rigor, pain in right side. May 29th, definite signs of consolidation of right lower lobe. Temperature, 104.5°.

Vaccine Treatment.—May 29th, 50 million stock vaccine. June 1st, 30 million own pneumococci, from sputum.

Temperature fell after each dose of vaccine, and on June 2nd, the fifth day, the temperature was normal, and remained so. The patient made a complete recovery. (Sir Almroth Wright's case.)



Case XIX.—A, Fifty million stock vaccine; B, thirty million own pneumococci.

CASE XX.

T. P., aged 23, male. No. 753. Admitted on May 6th, 1909. Onset on the 22nd, with rigor. On admission there were definite signs of consolidation in the right upper, middle, and lower lobes; also at the base of the left lung.

Vaccine Treatment.—On the 29th, seventh day, patient was given 30 million of stock vaccine. The temperature fell by lysis to normal on the 30th, the eighth day.

CASE XXI.

J. D., aged 11. No. 756, male. This patient was admitted for double mitral disease on May 28th, 1909. While in hospital on May 31st definite signs of pneumonia of left upper and lower lobes developed, with herpes, cough, dyspnoea, cyanosis, pain in side. There were definite signs of consolidation in the upper and lower lobes of the left lung. The pneumonia was of a very persistent type, and the temperature did not remain at normal until June 16th, seventeen days after onset.

Vaccine Treatment.—June 4th, 12 million stock pneumococci. June 6th, 20 million stock pneumococci. June 8th, 25 million stock pneumococci. June 9th, 30 million stock pneumococci. June 11th, 30 million stock pneumococci.

After each dose of vaccine the temperature fell, and there was a marked improvement. The respiratory symptoms entirely cleared up.

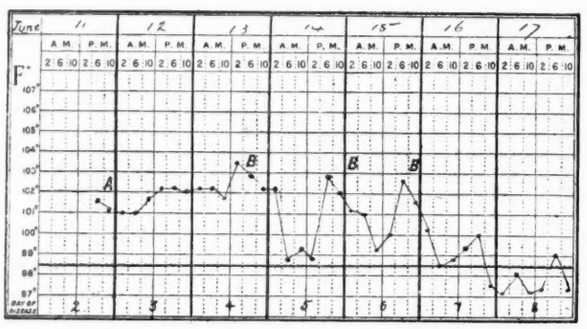
CASE XXII.

C. S., No. 810, aged 3 years 9 months. Admitted June 9th, 1909. Onset June 4th, with fever, cough, drowsiness. There were definite signs of consolidation in the lower lobes of both lungs, most marked on left side.

Vaccine Treatment.—Twenty million stock pneumococci given June 12th. Temperature fell by crisis on the eighth day a few hours after administration of vaccine, and all symptoms cleared up.

CASE XXIII.

W. W., No. 829, aged 40. Admitted June 11th, 1909. Onset June 10th, with rigor and severe pain in side. On admission there were all the symptoms of acute pneumonia, cough, rusty



Case XXIII.—A, Sixty million stock; B, fifty million own.

sputum, pain in side, pulse 120, respirations 48. There were all the signs of extensive consolidation of the right lower lobe.

Vaccine Treatment.—June 11th, 1909, given 60 million stock pneumococci. June 13th, 1909, given 50 million stock pneumococci. June 15th, 1909, given 50 million stock pneumococci. June 16th, 1909, given 50 million stock pneumococci.

After the last three doses there was a marked immediate fall in the temperature with striking improvements in the symptoms. The temperature fell by lysis to normal on June 17th, the sixth day of disease, and the symptoms completely cleared up. The case was a severe one, and the beneficial effects of the vaccine were very striking.

CASE XXIV.

I. P., aged 49. No. 861. Admitted June 18th, 1909, on the sixth day of disease. Onset five days before admission, with rigor and severe symptoms. On admission there were severe symptoms of pneumonia, with marked dilatation of heart and feeble pulse. There was marked delirium. There were definite signs of consolidation of the upper and lower lobes of the left lung, and also of the base of the right lower lobe. Patient died on the eighth day, June 21st, from cardiac failure.

Vaccine Treatment.—On June 19th, the sixth day, patient was given 40 million of stock pneumococci. No result, beneficial or otherwise, followed this dose.

At the autopsy there was marked grey hepatization of the whole of left lung and a patch of consolidation of base of right lung. The heart was dilated, the muscle soft and fatty.

The Clinical Effects of Inoculation.

In our cases we frequently found that there was a definite improvement in the clinical symptoms of the disease, thus there was often an immediate fall in the temperature, and the dyspnoea and delirium became less after a dose of vaccine.

The duration of the disease appeared in some cases to have been shortened; thus, out of our 24 cases in one the crisis occurred on the second day, in one on the third day, in one on the fourth day, and in three on the fifth day after the onset.

In some cases instead of a fall in temperature by crisis, the fever subsided by lysis; this happened in eight of our cases, a rather larger proportion, we think, than would have been the case without vaccine treatment.

The most striking and convincing evidence of the benefit derived from vaccine treatment was shown by those cases which ran a protracted course. Thus in Cases I, IX, XI and XIII, all of which were severe cases and ran a protracted course, the beneficial effects of vaccine were most striking, and these results could have been due to no other cause, since all the usual therapeutic measures had previously been adopted with no result.

We found in two of our cases, Nos. I and XI, that while the ordinary stock vaccine gave no benefit, an autogenous vaccine almost immediately produced most striking effect, the symptoms rapidly clearing up.

We consider it most desirable that vaccine treatment should be commenced as early as possible in the course of the disease, as then the beneficial effects are likely to be much greater.

DISCUSSION.

Mr. BUTLER HARRIS (London) observed that Dr. Willcox's results were in agreement with those published by himself in a paper in the issue of the BRITISH MEDICAL JOURNAL for June 26th, 1909, particularly with regard to cases of a prolonged nature. He agreed that large and frequent doses given between the fifth and seventh days, when a crisis might be expected, were contraindicated. In a case detailed in his paper the temperature rose steadily until death occurred on the eleventh day. As regards a vaccine, he was coming to the conclusion that perhaps an autogenous vaccine was not so vital as a vaccine derived from a virulent strain of pneumococcus. In a recent case the patient was ill and run down, and her sputum crowded with capsulated pneumococci. The speaker considered she was suffering from a mild pneumococcic toxaemia, and warned her to take care not to overtax her strength, as she might get pneumonia if not careful. Six weeks after this warning the patient went down with acute lobar pneumonia. A 25 million pneumococcus vaccine from a virulent strain was given at the end of the second day; an artificial crisis occurred within eight hours, and the patient recovered. Another case, where sputum could not be obtained, failed to respond, and died after a desultory illness of a month. A diffuse catarrhal condition was found in the lung, which was crowded with Friedländer cocci. In a vegetation on the mitral valve the *Micrococcus catarrhalis* was also found.

Dr. PARRY MORGAN (London) said: The difficulties in estimating the opsonic index in pneumonia suggests that there are several factors concerned. It had been found that with pneumococci that give no phagocytosis an extract may be made that inhibits phagocytosis, also that the organism, after the extraction has been made, is now acted on by the serum, phagocytosis readily taking place. This suggests that there are at least two factors concerned in phagocytosis, namely, opsonin which favours, and a substance in the organism which inhibits it. Again, organisms obtained from different sources behave differently as regards phagocytosis, the lung organism, for example, is very often not active whilst the mouth organism is. Again, culture makes a difference in this respect. This suggests that pneumococcus obtained from the lung has more substances against which immunity must be developed than the others. Again, we have found that the pneumococcus obtained from a patient is often agglutinated by the serum of that patient late in the disease, and frequently the serum of one patient will agglutinate the organism from another, but this is not general. We see, therefore, in cases of infection that immunization must be developed to the pneumococcus, to the toxin that inhibits phagocytosis and to the antigen that produces the agglutin. Thus it appears that the organism that possesses all these characteristics will be the one that will make the most effective vaccine. On the other hand, such a vaccine might be the most toxic. Some of the cases certainly show that whilst a stock vaccine is ineffectual the autogenous vaccine is not so.

AUTOINOCULATION IN PULMONARY TUBERCULOSIS.

By Marcus Paterson, M.B., B.S.Durh.,
Medical Superintendent, Brompton Hospital Sanatorium, Frimley.

First, in order to eliminate any ambiguity regarding my use of the word "autoinoculation," I wish to explain that by it I mean the effect of exercise on the blood as shown by general health, temperature, and the increase or decrease of sputum. In 1905, when I first prescribed graduated labour for tuberculous patients at the Brompton Sanatorium, Frimley, I had no idea that it would have any direct influence on the disease itself, but thought rather that work would materially improve the general condition of patients, and consequently they would avoid the lazy habits so frequently acquired at sanatoriums. After one year's experience I began to observe in a few patients that harder work caused a marked improvement while they had remained stationary on light work. This improvement was shown by an increase in weight and a decrease in sputum. At the time I concluded that the harder work had bettered the appetite and through that the entire condition.

In 1907 Dr. A. C. Inman explained to me Sir Almroth Wright's theory of autoinoculation, saying that he believed exercise given to patients had good results because it induced autoinoculation. Dr. Inman accordingly examined the blood of many patients undergoing graduated labour at Brompton Sanatorium, and the results of the investigations confirmed his opinion. From that time I have endeavoured to carry out the treatment of patients suffering from pulmonary tuberculosis along the lines of autoinoculation. This theory has brought me from working in the dark to working with open eyes, and has fixed a point at which to aim to find out how much useful work a patient can do without rise of temperature and with decrease of sputum. This seems to me a more practical object than the old method of fattening—compelling patients to continually rest and eat. Treatment by autoinoculation has the additional advantage of shortening the time necessary for a patient to remain in the sanatorium. Comparing the first six months of 1907, when I was not working in autoinoculation, with the same period in 1909, I find that 50 per cent. more patients have been treated at Frimley in the same building accommodation and without the addition of a single bed. I have elsewhere given the details of my system of graduated labour, and I should only like to point out that some patients from the first commence to have a decrease in their sputum, whilst others have no decrease at all until on fairly or very hard work, when the sputum entirely disappears, except very little in the early morning. This happens so frequently, that there is no doubt in my mind that it is due to the influence the work has on the blood—that is, to autoinoculation. In the short time at my disposal I do not propose to enter further on the subject of induced autoinoculation than to say that in practice I have found the autoinoculation caused by increase in the grade of work is not very great; but should it be greater than was anticipated, the overdose can be controlled by absolute rest, and the patient on recovery can start again on the same grade, with little fear of another excessive autoinoculation.

What I chiefly wish to impress in this paper, however, are the methods we have of controlling excessive autoinoculation. I think Sir Almroth Wright has laid down that spontaneous autoinoculation, which cannot be controlled, is incompatible with life. Irregular pyrexia in pulmonary tuberculosis is evidence of activity of disease, and is associated with irregular spontaneously occurring autoinoculations of toxin. Persistence of this fever and state of the blood can only lead to the death of the patient, and it is therefore obvious that every possible means must be taken to prevent the spontaneous autoinoculation which is causing the fever. To bring the patient's temperature to the normal is our object, and this is most readily accomplished by enforcing absolute rest. In the system of graduated labour patients who have a temperature as low as 99° F., taken in the mouth, *plus* a headache, are not only sent to bed, but are in addition treated in every way (with the exception of diet) like a typhoid patient; they are not allowed to wash themselves, to move in bed, nor to go to the water-closet. Every possible means are used to stop cough which does not result in the bringing up of sputum—that is, unnecessary cough, and in no case would a patient be examined with the stethoscope and made to take numerous deep breaths, nor be allowed to sit up in bed or to cough for the purposes of physical examination. The reason for these stringent regulations is obvious—namely, to stop all possible sources of further excessive autoinoculation when the patient is already under the influence of such autoinoculation.

When I was quite ignorant of the theory of autoinoculation, and a patient was worked to an extent resulting in a rise of temperature to 99° F., *plus* a headache, and the other subjective symptoms associated with a severe autoinoculation, he was sent to bed but was allowed to wash himself and to go to the water-closet. When thus treated the temperature commonly does not become steady for ten days or a fortnight.*

Since I have become more familiar with autoinoculations the patients have not only been sent to bed when they have had such a rise of temperature (99° F. in the mouth, *plus* a headache, that is, excessive autoinoculation) but in addition have been treated with absolute rest, with the result that, as a rule, the temperature becomes normal within three or four days. The patients in consequence are able to return to the work which caused the excessive autoinoculation generally within a week, and they can return to the work which caused the rise of temperature as the autoinoculation has made them immune to that dose. The patients often say that they feel better in themselves than they did before the rise of temperature; and at all events with these precautions mentioned they are never any the worse for the rise of temperature.

The difference made in these cases of ever slight departure from the rule of absolute rest is very great. If, for instance, a patient who is on absolute rest, and his fever declining, gets up against orders to go to the lavatory, his temperature chart is likely to show forthwith a reversion to the old type. The wisdom of absolute as opposed to comparative rest is further evidenced by cases such as the following: A male patient previous to his admission was kept in the fresh air at home and at rest, but not absolute rest in bed, and his pyrexial temperatures continued. On his admission to the sanatorium, absolute rest was substituted for this partial rest, with the result that in two days his temperature was perfectly normal. He rapidly regained his appetite, having had no relish for his food, altogether due to excessive autoinoculation; his constant cough and sputum, also due to the same cause, at once decreased, and within a week he was a different man.

If we wish to do anything for a patient with fever in pulmonary tuberculosis, we must keep to the fore in our minds, "How can I prevent this excessive autoinoculation?" Open air alone does not stop excessive autoinoculation, nor does a system of absolute rest, unless steps are taken to secure that it is truly absolute. This point is exemplified by a case in which the patient, though nominally on absolute rest, experienced for a considerable time no benefit as regards the fever. The reason was simple; the patient had an irritable cough, which had little to do with the bringing up of sputum. On controlling the cough by giving morphine, the fever rapidly disappeared and the sputum decreased. The morphine linctus was replaced by a simple linctus within ten days. It is obviously useless to insist upon absolute rest unless the coughing can be controlled, because the patient who coughs violently and shakes the whole bed is, as regards foot-pounds of work, taking fairly vigorous exercise.

It will be gathered from what has been said that I regard a temperature of 99° in the mouth, accompanied by a headache in an early case of pulmonary tuberculosis, as a warning of excessive autoinoculation. In women I consider 99.6° to correspond to 99° in men. I found that men with old or rather extensive disease often have a temperature of 99° or over, which is not of the same significance as 99° in early cases.

Sanatorium treatment at present can be summarized, I think, as follows:

1. Placing patients under ideal hygienic condition (the open-air treatment) and keeping them at rest.
2. The above *plus* artificial inoculations of tuberculin.

* This and other points in connexion with the course of pyrexia in autoinoculation described in the paper were illustrated by charts of cases.

3. Prevention of excessive autoinoculation.
4. Inducing autoinoculation by graduated exercise or work.

The first by itself undoubtedly does good, but it seems to me a combination of all four will give better results.

I hope that anything I have said as to absolute rest in pulmonary tuberculosis will be taken as advice in hopeful cases only, as I consider it a great unkindness to place any restriction on a patient whose life is ending from consumption.

DISCUSSION.

Dr. BUTLER HARRIS (London) drew attention to the fact that when, after prolonged treatment with tuberculin and rest, a normal opsonic index had resulted for some time, the patient must not be considered to be necessarily cured of tubercle for all time. He instanced the recurrence of disease after varying intervals from ten months to two and a half years in fresh sites, the patients having in the interval passed out of view. In other words, the tendency to infection in a certain type of cases was not removed. Possibly such patients ought to be inoculated regularly as a matter of routine for many years, perhaps even for life.

Dr. W. H. WILLCOX (London) thoroughly agreed with everything that Dr. Paterson brought forward. He believed that a thorough grasp of the principles underlying autoinoculation was essential if one was to treat successfully acute tuberculous disease, or, indeed, any acute infective disease. In the course of his work he had seen numerous interesting examples of autoinoculation. Thus, in cases of disease—for example, diabetes, where there was a very slight latent pulmonary tuberculous lesion—unusual exertion, or often simply a prolonged physical examination, would produce a rise of temperature by autoinoculation. Then, again, in several cases of gastric ulcer which had been under his care, where there was a slight latent tuberculous lesion of the lung, and where the operation of gastro-enterostomy had been performed, the autoinoculation produced had caused pyrexia for from one to two weeks after the operation. It was well to warn surgeons of this possibility in similar cases.

Dr. CROFTON agreed that it was useless to depend solely on the effect of open air. Both in acute and chronic forms of phthisis he had had complete cures by intravenous injections of iodoform in ether.

ON TYPHOID CARRIERS.

BY THOMAS HOUSTON, M.D.
(From the Pathological Laboratory, Queen's College, Belfast.)

As the work I have been doing on chronic typhoid infection is in a very unfinished condition, I present this paper as a mere preliminary sketch of some points of interest and as a record of some suggestive cases.

CASE I.

In 1898 a female patient was admitted to the Royal Victoria Hospital under the care of Sir John Byers. The typhoid bacillus was isolated in pure culture from her urine, and her blood gave a well marked Widal reaction. As the case was fully reported in the BRITISH MEDICAL JOURNAL of January, 1899, attention here need merely be drawn to certain special points:

1. There was no history of an acute attack of typhoid fever.
2. The symptoms of cystitis, severe headaches, and general ill health required hospital treatment for months, and had persisted for at least three years.
3. This case appears to be the first recorded case in which typhoid bacilli were shown to be voided in the urine for a long period.
4. Urinary antiseptics and other methods of treatment were without effect in diminishing the number of bacilli in the urine.

The patient, who in view of the facts noted under paragraph 2 cannot be described as a healthy typhoid carrier, left hospital without improvement and was lost sight of. She is probably, if alive, still disseminating typhoid bacilli.

CASE II.

The case has as sequence a house epidemic.

The patient, K. M., aged 26 years, while a servant in a house in Liscard, contracted typhoid fever seven years ago, and was confined to bed for seven weeks. After her recovery she took another situation in Liscard, and in a month's time, about the end of March, her master developed typhoid fever. The next two years she passed in different situations, nothing of importance occurring. She then took a situation in Liverpool, where the son of her mistress developed typhoid fever in March or April. Finally, she came to Belfast, and while in service in this city four people, A, B, C, and D, in the same house developed typhoid fever at short intervals (June to July). In other words, in the course of seven years six people, living in the same houses as the patient, developed typhoid fever. Dr. S. T. Irwin suspected that she might be a "typhoid carrier." Questioned as to her attack of typhoid, the girl at first stated that her recovery was complete, and she was soon able to return to work, but on cross-examination she admitted that since her illness she had not been in robust health. She was easily tired; she suffered from frequent headaches and occasional gastric disturbances. Her general condition evidently varied, but her health was not altogether satisfactory. He had her blood examined. It gave an imperfect Widal reaction in 1 in 50 dilution in one hour, and a good reaction in lower dilutions. She was therefore advised to go into hospital that her condition might be investigated.

She was admitted to the medical wards of the Royal Victoria Hospital on July 18th, 1908, and was placed under the care of Dr. Willlam Calwell. A catheter specimen of her urine was obtained; it was opalescent, acid in reaction, had a specific gravity of 1022, and contained a trace of albumen. The centrifugalized deposit contained pus cells, bladder epithelium, and an enormous number of Gram-negative bacilli. The urine was then plated on Conradi-Drigalski medium, 1 c.cm. and 0.5 c.cm. of urine being placed respectively on each of the two plates. After the urine had been evenly distributed over the surface of the plates any excess was poured off. This procedure was adopted in all further examinations of the urine of this patient. The next day both plates were studded with innumerable colonies. These on further examination gave all the typical cultural reactions of Eberth's bacillus. Several samples of typhoid blood which were found to agglutinate the laboratory strain of typhoid also agglutinated this bacillus, recently isolated from the urine, in dilutions of 1 in 10, 1 in 50, and 1 in 100. The agglutination was not so complete in the high dilutions as the agglutination of the laboratory strain. The blood of the patient agglutinated the laboratory strain well in dilutions of 1 in 10 and 1 in 20, but only partially in a dilution of 1 in 50 in one hour. The bacillus isolated from the urine gave a good reaction with a 1 in 10 dilution of the patient's blood, but in higher dilutions the reaction was imperfect. Several examinations of the urine and blood were made, with the same results as those detailed above. In other words, the urine of the patient contained the typhoid bacillus in pure culture and in enormous numbers, while her blood gave distinct evidence of infection. The faeces were also examined by cultural methods for typhoid bacilli, but none were found. The patient's temperature was found to vary from 98° to 100° F.; with a two-hourly chart the temperature was seen, as a rule, to touch 100° between 12 noon and 4 p.m. On admission her weight was 8 st. 13 lb. For the first five weeks she was treated with tonics, urinary antiseptics, and rest. During this period no improvement was noticed. Her weight fell from 8 st. 13 lb. to 8 st. 11 lb.; she still complained of gastric pain and of headaches; her temperature range was the same as on admission; from the same quantities of urine quite as many colonies developed. A vaccine was now prepared from the typhoid growth which had been isolated from her urine. It was standardized and sterilized in the usual manner. The opsonic index to her own bacillus was determined on several occasions by the formalin method and found to vary between 2 and 3. It was an easy matter to pick out this patient's blood from a series of controls by this opsonic test.

On August 25th 50 million sterilized typhoid bacilli were inoculated. This caused a higher range of temperature for a day or two and considerable general malaise and headache. On September 1st the urine was plated out and numerous typhoid colonies developed.

On September 2nd 100 million sterilized typhoid bacilli were inoculated; the general malaise, etc., were less marked than on the first occasion. On September 16th the urine was plated out as before, and the following is the note written on that date: "The colonies which developed on the plates very much fewer in number than in any previous occasion on which the urine had been plated out."

On September 20th 200 million were inoculated. On the 22nd we were at first disappointed to find on plating out the urine as before that quite as many colonies developed on the plates as ever before. The patient was now put on sodium lactate and her urine was made alkaline in this way. On October 8th no

typhoid bacilli developed; the urine was incubated, but only a staphylococcus was found in the urine thus incubated.

On October 9th 300 million were inoculated. On the 20th no typhoid colonies grew from the urine.

On October 23rd 500 million were inoculated. On the 24th the urine was absolutely clear; no pus, no albumen, no colonies developed on the plates. On November 21st the patient returned from the country to report herself, and was kept three days in hospital. No typhoid bacilli were found in the urine or faeces.

On November 21st 1,000 million were given. On the 23rd no typhoid bacilli grew on the plates. The faeces were again examined with a negative result. Her blood now gave a well-marked Widal reaction in dilutions of 1 in 50, 1 in 100, and 1 in 200 in one hour. During the period in which the patient was being inoculated she gradually improved in general condition and weight; her weight increased from 8 st. 11 lb. to 9 st. 7 lb.; her headaches, the feeling of tiredness, and her gastric pain all disappeared. It was noted by weekly weighings that immediately following the first two inoculations the patient lost a little in weight—the first two inoculations produced more general malaise than any of the others. Since November she has reported herself in December of last year, and again on April 24th, 1909. The urine and faeces were examined with negative results. She has been back again at work for months and appears in good health.

The after-history of the four persons, A, B, C, and D, who were apparently infected in Belfast by this case, is as follows:

A (the master of the house) appears to have made a good recovery.

B (a second servant) remained in poor health for six months after her recovery, suffering from malaise, headache, etc. She promised to come to hospital for examination if her health did not improve, but never reported herself, so she has probably regained her health.

C (a hospital nurse) made a slow recovery, and six months afterwards was still suffering from headaches, easily tired, and feeling unable for work.

As her blood still agglutinated the typhoid bacillus in 1 in 50 dilution, she was brought to hospital and the typhoid bacillus was recovered from her faeces in large numbers. (These appeared to clear up under vaccine treatment. She is now back at work and apparently in good health, though not very robust.) Her opsonic index from typhoid fever was originally below normal, and before the inoculations were stopped was keeping above the normal line. Her temperature was subnormal while in hospital.

D (the lady of the house) was consumptive, and had a tuberculous deposit in her apex and some tuberculous glands in her neck. She never made a good recovery from her illness, and six months afterwards she developed a temperature and complained of frontal headache.

Her symptoms were first supposed to be the result of tuberculous mischief. A few small inoculations of tuberculin were given without much effect. Her temperature and the peculiar type of her headache suggested that she might still be infected with the typhoid bacillus. After some difficulty the typhoid bacillus was isolated from her faeces. Several inoculations with typhoid vaccine were then given with very good effect. Her temperature quickly fell to normal, and she appeared in good health and remained so for three months. About the end of March she again became ill, and during April developed an acute febrile illness, with slight cough and some signs of bronchitis. For some time it was difficult or impossible to determine whether this temperature was due to the typhoid or tubercle bacillus or to both. Her blood still agglutinated to the typhoid bacillus to a slight extent, but we failed to isolate the bacillus again.

Unfortunately the progress of the case made it evident that at least one of the factors in her illness was a recrudescence of the tuberculous trouble in her lung, and she was sent to a sanatorium.

Case III.

This patient had been in poor health for four years, suffering from symptoms pointing to some chronic infection—namely, irregular temperature, varying from normal to 100°, or occasionally 101°. This condition had persisted for at least four years.

The temperature was subject to fairly regular exacerbations every fourteen days or thereabouts. She complained of severe frontal headaches and occasional chills. She was thought to have antral disease, but, although a small abscess, probably connected with a carious tooth, was found on operation, nothing to account for her condition could be discovered. Various forms of treatment were tried without any benefit. She was believed to be tuberculous, and a course of tuberculin advised. I was unable to obtain any evidence of tuberculous infection from her blood or otherwise. Small inoculation of tuberculin had no good effect.

Her sister, on her return home after an absence of several weeks, developed an acute febrile illness, which was supposed to be influenza or typhoid. It ran a course of three or four weeks of high temperature, and during convalescence all her hair came out. Her blood was examined on the third day of her illness. There was no leucocytosis and no typhoid reaction. Several months after her acute illness, as she was still in indifferent health, her blood was again examined. It now gave a definite Widal reaction, and the typhoid bacillus was recovered from her urine. Her sister, whose condition was the same as before, was also found infected with the typhoid bacillus, and her blood gave definite signs of infection. These two patients are at present being treated with typhoid vaccine. The girl who had the recent attack of fever is greatly improved, and is probably free from the infection. Her sister is still ill, but appears to be slowly improving.

The above details of these cases show that a condition which may be described as "chronic typhoid" does occur, and that typhoid carriers may suffer from symptoms pointing to a chronic infection. The most characteristic feature found in this series of cases was a peculiar frontal headache which recalled the headache of the first week of typhoid fever. This was marked in all the cases except one.

All the cases, except the first, mentioned above, have been treated with vaccines made from the bacillus isolated from the individual cases, or at least from one of the groups of cases under consideration.

Of these cases, two only can be described as chronic typhoid carriers, the others were still infected several months after an acute attack of typhoid fever.

Of the two chronic cases, one is apparently free from infection, but the other is still under treatment and has improved to some extent. Of the cases of recent infection all have improved greatly, and may be free from typhoid infection, but when we consider the great difficulty in coming to a decision in such a matter, especially when frequent examinations are impossible, we can only state that the symptoms of infection in such cases disappeared under treatment. One of the greatest difficulties in such cases, especially when the bacilli are found in the faeces, is to determine when the patient is entirely free from infection. This can only be done by frequent and repeated examinations. It may also be found of value to give the patient a fairly large dose of typhoid vaccine, and if the bacilli are still present they will probably increase in numbers during the negative phase, and thus be more easily found. This was found to be true in the case of K. M.

Determinations of the opsonic power of the blood are made in all the cases described above. In two cases it was found above normal—that is, the cases with subacute temperatures. In the two with normal temperatures it was below normal. The determination of the opsonic power with regard to typhoid presents the following difficulty: the bacilli are susceptible to an agglutinative and bacteriolytic action for the serum. Nevertheless, with recently isolated cultures grown for a few hours fairly consistent results were obtained. Dr. Rankin and I, when working with the meningococcus, found that after prolonged subculture a meningococcus may lose its power of indicating by a high opsonic index the presence of infection. This result is due to the fact that as the organism becomes attenuated by artificial growth it gradually loses its resistance to the opsonic action of normal serum. This has been shown to hold in the case of some varieties of *Bacillus coli* by Wright. My results indicate that the typhoid bacillus behaves in a similar way. In determining the opsonic power of the serum of patients infected with typhoid it is useless to work with an old laboratory culture. A recently isolated strain should always be used.

A line of research which seems to be of great importance suggests itself in this connexion. Are there different strains of typhoid bacilli? or are the observed differences due to the attenuation of the micro-organisms through growth on artificial media? The answer to this question will determine the best method to prepare a typhoid vaccine. It seems quite certain that ordinary antityphoid vaccine which is made from a frequently subcultured bacillus has not the same potency as an autonomous vaccine. Is this a question of subculture, or are there real differences in the strains, as in the case of coliform organisms? Our work on this subject has not gone far enough to definitely answer this question. A recently isolated culture from the case was used in all the determinations. A few hours' growth was employed, as this

seemed to reduce the bacteriolysis to a minimum. The general law of negative followed by a positive phase was observed. It was found in cases with even a slight temperature that 50 million was probably an excessive initial dose, as it frequently produced considerable general disturbance.

From the experience I have had of these cases, I am of opinion that the clearing up of a chronic typhoid infection which has persisted for several years may prove an exceedingly difficult task, though my investigations in the case of K. M. seems to show that it is quite possible. On the other hand, if the cases be inoculated a short time after their initial attack of typhoid, the infection will usually be quickly eradicated. The critic will of course ask, Would this not have occurred without inoculation? In many cases it would; in a certain percentage it would not. Cases of typhoid, where the convalescence is slow, will derive great benefit from such a course of treatment, and if the infective period of the disease be thus shortened a distinct advance will have been made.

DISCUSSION.

Professor MCWEENEY (Dublin) said that during the past year he had traced the causation of two outbreaks of typhoid to carrier cases. In one case the carrier was a girl employed in a dairy from which milk was sent to a girl's school, where 35 of the pupils were suddenly struck down with typhoid. Although apparently in good health her serum was found to agglutinate the typhoid bacillus in a dilution of 1 in 500. On inquiry it was found that two years previously she had had an illness which resembled typhoid. In the other case, which was also milkborne, several hundred persons had become infected from a dairy where on blood examination several persons were found with high Widal reactions. The question which he wished to raise was whether the agglutinative power of the blood was always raised in carrier cases so that it might be relied upon as affording an indication of the cases whose excreta it was necessary to examine.

Professor WALKER HALL (Bristol) said that the affective periods of typhoid carriers were very variable, and that in the present condition of our knowledge it was difficult to know whether the disappearance of the typhoid bacilli from the excreta was voluntary or due to the effect of any form of treatment. Minute doses of vaccine, such as 25,000 (not millions) in his experience acted equally well with larger doses. He thought that with regard to agglutinative reactions these should be quoted with regard to some standard, and he hoped to make some suggestions in this regard at an early date. Although vaccines apparently produced some action in these cases, it was difficult as yet to attribute any direct action. In order satisfactorily to exclude the periodic factors, and to put the vaccine therapy on a sure basis, the excreta of "carriers" ought to be examined daily for at least two years. This was being done in a case under the care of the Bristol Health Department.

THE ANTITRYPTIC INDEX.

By F. L. GOLLA, M.A., M.B.Oxon.,
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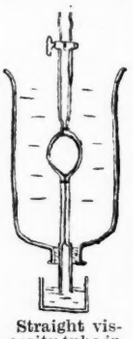
IN a paper published in the *Lancet* last April I was able to show that by using a method depending on the increase of electro-conductivity of a casein digest it was possible to express the value of the antitryptic index of normal and pathological serums with the greatest accuracy, so long as the observations were so arranged as to fall in that stage of the curve of velocity reaction when the relationship of two digests is an arithmetical one. The results obtained were expressed in a ratio which may be either + or −, and which represents the relative strength of the antitryptic power of a pathological serum as compared with normal serum. At the same time it was shown that results obtained by use of a serum plate method or the end-point reaction of Fuld, owing to these methods neglecting the fundamental requirements of a comparative observation of enzyme action, yield contradictory results, and only can be relied on to show the most gross differences of antitryptic power. I need not deal further with methods, except to mention another physical one which I have recently adopted and which, for its accuracy and simplicity, should prove very satisfactory.

This method for estimating a tryptic digestion was devised by Spriggs, and depends on the changes of viscosity taking place during the progress of digestion. Spriggs showed that curves obtained by this method bore a close relationship to the curve of the diminution of coagulable proteid. Bayliss, who is the only observer who has repeated the experiments of Spriggs, showed that the viscosity change is generally brought to a standstill long before the curve of the increase of conductivity has advanced more than a very small degree. This was only to be expected, inasmuch as the increase of conductivity depends to a great extent on the formation of amino acids, a change taking place comparatively late in the course of a tryptic digestion. Nevertheless, though one is measuring two different stages of reaction, I have found a close concordance between the results obtained by the two methods, and though a little less sensitive than the electrical one, the viscosity method yields very reliable results.



Straight viscosity tube in water jacket dipping into a beaker containing digest.

I use the straight viscosity tube shown, and inasmuch as one is only determining the relative changes of viscosity in preparations treated with different serums, and since these are all under the same conditions, the temperature may be neglected as far as the digests are concerned, and the experiment carried on at room temperature. By arranging the strength of ferment used and the alkalinity of the medium so that the observations fall within the early period of the curves when the law of simple arithmetical relation holds, it is possible to obtain correct results with a single initial and a subsequent observation and the time occupied need not be above twenty minutes. The viscosity tube is protected from changes in temperature in handling by a water jacket, and, with the exception of a stop watch and a rubber suction bulb for filling the tube, no other apparatus is required.

To pass now to the results obtained, it is only possible in the time limit assigned to offer a general summary of them. The possibility of estimations of the antitryptic index being of any pathological use is so necessarily connected with the question of the constancy of the antitryptic power of normal blood that the series of investigations previously reported have been much extended. Altogether 40 normal cases have been compared, and in none of these was any divergence from the normal found that lay outside possible slight errors of experiment.

Starvation had no effect on the index, even in cases of gastric ulcer from whom food had been withdrawn for three or four days. The index is apparently unaffected by the age of the patient, but I have not tried the serum of young babies.

Having established the constancy of the normal index, attention was turned first of all to bacterial diseases. As the most promising of these, on account of the length of its duration and the ease with which inoculative experiments can be conducted in it, most of the work has been done on tubercle. In my former paper I have published a table of 25 cases to show that the temperature in tuberculosis has no bearing on the antitryptic index. It is true that if prolonged variations of temperature occur in the same individual his antitryptic index will also vary, but it is impossible from inspection of the temperature chart of a tuberculous patient to say whether the antitryptic index will be greatly or only slightly raised. The first impression gained, therefore, was a rather confused one as to the value of the index. In about 80 per cent. of the cases of tubercle the index was more or less raised; in the remainder it was normal, and in 2 cases subnormal. In all, 53 cases have been examined. In some cases the index was as high as + 0.8, in others only raised to + 0.02.

A clinical survey of the cases, however, conveyed the impression that those cases in which the index was very markedly raised were doing badly, and showed little tendency to improve or recuperative power after some such exhausting process as a bad night, whilst those cases in whom the index was only slightly raised showed a tendency

towards recovery. In order to test this impression it became necessary to follow out a number of cases, noting the relationship of their general symptomatology to the antitryptic index, and further, to trace the effect on the antitryptic index of inoculations with tuberculin. I have detailed elsewhere cases in which these two methods were combined, and since then have added further cases on whom observations extending over some weeks were conducted. In addition, the results of tuberculin inoculation have been studied in a series of 42 inoculations, the antitryptic index having been observed both before inoculation and at varying periods from two to twelve hours after.

As most of these cases have been reported elsewhere, I will only summarize the results arrived at. First, in cases which improved there was a tendency to a progressive decline of the antitryptic index, and in two cases which showed arrest of the disease the index became practically normal. On the other hand, in two cases in which the index to start with was very high and the patient progressively became worse, the index increased slightly, rising in one case from + 0.2 to + 0.4 in the course of two months' observation. In a case which was followed out for three months in a nursing home an index of + 0.2 became progressively lowered to + 0.08, during which time the patient steadily improved, but a bad relapse occurring some two months later the index was found to have risen to + 0.4. In one of these cases detailed in my paper the patient was afebrile nearly all through a progressively increasing pulmonary involvement, and the index, which was high at the beginning, showed an increase which bore no relation to occasional brief bouts of temperature.

Secondly, in the 43 cases of inoculation studied, a rise of the antitryptic index followed the inoculation in 33. Here the interesting fact was elicited that the amount of rise after an inoculation varies inversely to the height of the antitryptic index in the patient as regards normal before inoculation. To illustrate this point I append a list of 4 cases with different antitryptic indices, each of whom received 0.001 gram of tuberculin. It will be seen that where the index was high the response was comparatively small.

1. Antitryptic index = + 0.5. Index of serum after inoculation to that before = + 0.07.
2. Antitryptic index = + 0.4. Index of serum after inoculation to that before = + 0.1.
3. Antitryptic index = + 0 08. Index of serum after inoculation to that before = + 0.2.
4. Antitryptic index = normal. Index of serum after inoculation to that before = + 0 22.

Again the strength of the dose elicited a proportionate response as in this case: $\frac{1}{2000}$ mg. tuberculin—rise = + 0.05; $\frac{1}{1000}$ mg. tuberculin—rise = + 0.2.

In a case under the care of Dr. Latham no rise of antitryptic index followed administration of $\frac{1}{2000}$ mg. tuberculin with 5 c.cm. of horse serum; when a few days later the dose was repeated with the serum omitted a rise of + 0.4 was noted. This confirms the clinical observation of Latham that tuberculin administered with normal horse serum by the mouth has much less effect than the same dose without serum. In the 10 cases in which inoculation had no effect the dose was less than $\frac{1}{5000}$ mg. in 5, and in the remaining 5 the index was very high. As a matter of clinical observation, it may be stated that wherever the index was very high and the patient inoculated, the effect of the dose only appeared to aggravate his symptoms. In three patients dying of pulmonary tuberculosis in whom the antitryptic index was estimated a few hours before death it was very high in all, being + 0.6, + 0.75, and + 0.7 respectively.

The time relations of the antitryptic rise have so far only been roughly determined. The earliest period in which it was noted was two hours after an injection and four hours after administration of tuberculin by mouth. After about forty hours the index again sinks to its previous height. Nothing in the shape of a negative variation has been so far observed.

It would seem, therefore, that in pulmonary tuberculosis this rise of antitryptic power affords an easy method of determining the effect of an inoculation. In several afebrile cases, where the temperature chart has shown no result from inoculation, the antitryptic index has shown a well-marked rise. The temperature chart at the best, when it indicates a reaction at all, cannot quantitatively express the degree of the response; it would appear to bear the same want of relation to the degree of stimulus that the report of a gun does to the force with which the trigger is pressed. In addition, the antitryptic index, besides telling us the effect and the necessity for performing or abstaining from inoculation, may have some value in reinforcing clinical observations of the progress and prognosis of the disease.

In 2 cases of pure surgical tuberculosis involving the joints I was unable to find any alteration of the antitryptic index, nor was there any rise following inoculation. In neither, however, was there any temperature nor any constitutional symptoms.

As to the value of the antitryptic index in diagnosis of tubercle, we are on less explored ground. In 2 cases of normal people inoculated with tuberculin there was no effect on the index; in the comparatively large percentage of failures to observe a rise of index after inoculation, the index was already high in all those in which inoculation failed with a relatively large dose. I am, therefore, inclined to think that in an early case of tubercle with a low or only slightly raised index the chances of a post-inoculative rise would be very great.

With regard to other infective diseases, observations have, so far, been rather scanty. I am at present engaged with Dr. Bailey on a systematic examination of the cases in St. George's. Nine cases of pneumonia have been examined during the height of the fever, and of these the antitryptic index was again examined in 6 after the crisis. In all cases, whilst the fever was at its height, the antitryptic index was very high and it became only slightly lower in serum taken a day after the crisis. The descent appears to be very slow, and normal is only reached when the pulmonary disturbance has completely subsided. In a number of cases of scarlet fever the index was high during the febrile period, remained so after the temperature had come down, and in severe cases only reached normal about fourteen days after the fall of temperature.

In 2 cases of typhoid examined the index continued high two and three weeks respectively after the temperature had fallen. Both these cases showed signs of intestinal ulceration.

In 2 cases of malignant endocarditis the index was very high, going up markedly shortly before death; continuous observations were not, however, made.

In a case of diphtheria the index was only moderately raised, and fell with the temperature after administration of antidiphtheritic serum; the case was a mild one.

Late secondary and tertiary syphilitic cases have the index unaffected. Dr. Bailey is at present engaged in investigating the index in early secondary cases.

I have observed no affection of the index in the 3 cases of mild gonorrhoea I have examined.

In 5 cases of parenchymatous nephritis the index has been found to be raised. In 2 of these cases there was a recent history of acute nephritis; the history of the other cases was unfortunately not obtained.

The effect of cell injury is suggestive. In 3 cases after operation for hernia, which were doing well and were perfectly aseptic, the antitryptic index was raised. Similar results were obtained in 3 cases of fracture which was simple, and a quiet absorption of débris was probably proceeding.

One of the most curious and important variations of the antitryptic index occurs in the serum of patients affected by neoplasms. The increase of antitryptic power is so gross that it can be in most cases readily demonstrated by the plate method or that of Fuld, and Brieger and Trebing, using the latter, found that in a great majority of cases of carcinoma the index was raised. Their results have been confirmed by other observers, including Becker, but the nearly universal occurrence of this rise, when cases of slight rise are taken into account, requires for its demonstration the use of more delicate methods. In 15 cases examined I found only one certain exception to the rule that in patients with neoplasm the antitryptic index is raised.

For permission to use his very extensive observations I am indebted to Dr. Hort, who is reading a paper on the subject in the Surgical Section. This observer collected

100 cases of neoplasm, and found that in only 6 per cent was the heightened antitryptic index absent. After a variable time following complete removal of the growth the index fell to normal, and was found again to be raised when recurrence took place. His estimations of antitryptic power were done by Sörensen's method, which, if less delicate or easy than the physical methods I have mentioned, is quite sufficiently sensitive for this purpose.

The importance of these observations as an aid to the diagnosis of neoplasms cannot be overestimated. It is true that in cases where there is evidence of bacterial disease or parenchymatous nephritis the method is unavailable; but in the majority of cases where we are dealing with symptoms referable to pressure or obstruction in an otherwise healthy man it would appear that rise of antitryptic index is of almost diagnostic significance, and this rise will be of the greatest assistance in investigating the question of recurrence. Unfortunately there appears to be no discrimination in the antitryptic index between benignant and malignant growths.

Finally, a word may be said concerning the significance of the antitryptic index. Here our knowledge is practically *nil*. That the antitryptic power of the serum is attached to the albumen moiety was shown by Cathcart and Landsteiner, and the views of Glaessner, who considered that it was attached to the euglobulin fraction, may be considered to have been definitely disproved by Cathcart. A similar antitryptic power has long been known to be possessed by egg-white. Attached to the globulin portion of the serum is an auxilytic and possibly feebly proteolytic body, which was described by Hedin in the serum of the ox, and has since been noted by many observers. The antitryptic power of serum would appear, therefore, to be the algebraic sum of the antitryptic and the auxilytic bodies. It became, therefore, necessary to know whether variations of the antitryptic power were due to increased or decreased activity of the antitryptic moiety or of the auxilytic moiety. The fact that the antitryptic power is destroyed at 56° C. and the auxilytic power is left untouched enabled me to test this point by the simple method of taking two serums with high and low antitryptic indices, and, after estimating their indices, to use fresh portions of each serum which had been previously heated half an hour at 56° C. In the latter case the amount of tryptic digestion in the digests containing the two heated portions was exactly the same; consequently the variations of antitryptic power were due to the antitryptic content of the two serums. It may be further added that the auxilytic power is so feeble that its activity cannot be demonstrated in the small amounts of serum used and the time during which the observations are conducted. The question as to whether the antitryptic power is really a function of the albumen or of some body bound up with it is the subject of some work on which I am at present engaged. I have found it possible to extract the antitryptic body of serum with chloroform, and a lipoid body soluble in chloroform and ether and insoluble in acetone, possessing feebly antitryptic properties, has been obtained. Experiments are not, however, sufficiently advanced to enable any definite statement to be made.

The universal presence of potential intracellular proteolytic enzymes might make it seem likely that the serum inhibits a constant inhibitory action on such enzymes, and by an increase or decrease of inhibitory power regulates tissue metabolism. The heightened antitryptic power would be in these terms what the Germans have called a "cachexial reaction." The evidence against this view is very strong. In cases starving from non-malignant oesophageal stricture or gastric ulcer there is no rise of index, nor is the rise in malignant disease in any relation to the presence or absence of cachexia. More may be said in favour of the view that a rise of antitryptic index is a response to the introduction of tryptic enzymes into the blood stream liberated by autolysis of cells. This would explain the heightened index in cases where an aseptic operation or a simple fracture has caused cell destruction. I have, moreover, been able to markedly raise the antitryptic index in rabbits injected with rabbit liver and kidney which had been allowed to undergo aseptic autolysis for several hours, and then the ferments extracted by Buchner's method and injected. In such animals the serum showed a slightly increased inhibitory power on the progress of autolysis of normal rabbit and liver and kidneys as compared with that of the serum of a normal rabbit. But in the case of rabbits injected severally with liver or kidney this inhibitory power was not specific for that organ. The rise of index so sharply following inoculation of tuberculous subjects with tuberculin might be explained as due to an increased cytolysis, the liberated enzymes calling forth more antibody. It is hard, however, to reconcile such a view with the heightened index in rapidly growing malignant tumours, such as a fibroma of the uterus. It is hardly possible that in bacterial diseases the increased antitryptic power is due to the products of the bacilli themselves, though the antitryptic power of the bacterial body is amazing, as may be readily shown by growing culture in a trypsin solution kept at full strength by constant additions of fresh trypsin. It is urgently to be desired that some observations on the correlation of the leucocyte count of the blood and the antitryptic index should be performed. A paper on this subject has been published by Wiens, who found no striking concordance, but the unreliability of the plate method he uses allows no safe conclusions to be drawn from this work. I have not dealt with the relation of the antitryptic index to the phagocytic power of the blood, but this also is a subject offering a promising field for research.

DISCUSSION.

Mr. A. FLEMING (London) said that he was in agreement with Dr. Golla that in all cases of pneumonia the index was very high, and remained so after the crisis. In tuberculous cases he found that in cases in which there was apparently no secondary infection there was no rise of the antitryptic index, but in cases in which there was secondary infection with pyogenic organisms there was increase of the antitryptic power. He suggested, therefore, that the antitryptic index had no relation to the tuberculous infection, but was in relation to the pus-forming organisms which form the secondary infections.

GENESIS AND TREATMENT OF THE MALIGNANT TUMOURS.

By Professor SANFELICE,
Bologna.

WE can divide those who work on this subject into three categories.

To the first belong the pure morphologists, who, if they have rendered a great service to science in some ways, have been wrong in believing that the question of cancer genesis can be solved by dividing the carcinomata into hundreds of sections and studying their morphology and the genesis of the cellular neoplastic elements from the beginning to their complete evolution. They do not care for experimental methods, and violently attack those who, in consequence of experiments, have adhered to the parasitical theory of malignant tumours, denying them even the intellectual capacity to understand what may be a cancer and what may be a sarcoma.

These morphologists have laid down some theories with which they believed that they had explained the genesis of cancer, but without foundation, because when we have learnt that the cells of the organism which give origin to malignant tumours become anaplastic, or are displaced by inflammation in the connective tissue, we do not quite know the etiological agent of the anaplasia and of the inflammation that has afterwards produced the misplacing of the cellular elements.

At present the morphological study of the malignant tumours of man has given all it can.

To the second category belong those observers who also, without neglecting morphological researches, have followed the experimental way, transplanting the tumours from one animal into another of the same species or of different species, studying the more important conditions of development of the neoplastic cells, and seeking to solve the problem of the innate or acquired immunity against the growth of the transplanted cells. In these last years almost all the experimenters have fallen into a real mania to procuring themselves a rat or a mouse with a spontaneous tumour for initiating the series of the transplantations through many generations, as if this method of

research could solve the question. All these trials have taught us very little, and they have not advanced the question of the etiology of the malignant tumours; on the contrary, the result of all these transplantations of tumours from one animal into another of the same species or of different species marks a regression in the way of the important question, because the greater part of the observers tend to elevate the neoplastic cell to the degree of etiological agent by itself without the intervention of a parasitical agent, returning thus to the ancient conception of Cohnheim.

To the third category of observers who study the question of cancer belong the partisans of the parasitical theory, grounded on the pathologic action of the blastomycetes. It is a legion of very few labourers if we subtract those who, publishing researches made with little scientific rigour, have contributed to throw discredit on the works which deserve to be considered. One knows already by the researches acquired by science the power of transplantation of malignant tumours from one animal into an animal of the same species. And animals which have overcome a first inoculation show afterwards a resistance to the consecutive inoculations of tumours. The spontaneous tumours, like those obtained by transplantation, are, however, the secondary effect of a cause which has remained till now unknown; thus the study of the cells alone which form the tumour, which is the principal object of the morphologists, although important, could not form a step to the knowledge of the causes—that is, of the agents which are able to make the cells neoplastic; that is to say, to produce a tissue of new formation, a guest pernicious enough for the organism, and at the same time an essential and dangerous condition of every malignant tumour.

From the researches that I initiated at Cagliari in 1894, I found that in malignant tumours of man and of animals one meets pathologic saccharomycetes which can rarely be artificially cultivated, and are often uncultivated, because they are transformed into Russel's atoms for the action of an antibody, that forms itself in the blood of the animal, a prey to the infection. This in the same manner as the bacteriologic serums acts on the saccharomycetes, producing in them the fusion of the chromatic substance and the interposition of the parasites in very small round bodies. This transformation is owing to the organism possessing power of neutralization and reparation that are directed to hurt the life of the invading microorganisms. There is another important fact which resulted from the first researches—namely, that when the blastomycetes which we meet in the malignant tumours can be artificially cultivated, they present themselves coagulated, exactly like those forms which were considered by Soudakewitch, by Prusser, by Plimmer, and by others as belonging to protozoa which must not be confused with the usual cellular inclusions and with ordinary degenerations of the cellular protoplasms. This second fact represents the exception, because in the greater part of the malignant tumours the blastomycetes exist, but under the form of certain atoms.

The blastomycetes isolated from the malignant tumours inoculated alone into guinea-pigs and into rabbits give place to the production of tumours, which are for the greater part constituted of parasites. In this case one treats of pseudo-tumours, and not of real cellular neoplasms. These pathogenes blastomycetes inoculated alone into dogs in fewer numbers, but with a percentage superior to those we observe in the spontaneous malignant tumours, give place to a production of real epithelial connective tissue and malignant neoplasms. With this first series of vicissitudes one has not exhausted the study of the pathogenic power which the blastomycetes are able to exercise in animal organisms.

From a series of preliminary researches I was convinced that the cells of the malignant tumours, spreading far from the primitive tumour into the organs and forming the metastasis, do not transport the parasites into their cellular protoplasms. From that we deduce that the cancerous and sarcomatous cells must necessarily bear a stimulus represented by some chemical substance existing in them by which they can be multiplied at a distance from the primitive tumour. One must for that reason think of the existence of special toxins produced by the blastomycetes, and able to stimulate the cellular elements to the atypical reproduction *till* the formation of the malignant tumours.

Some years ago, while I practised a series of inoculations of the trachea on guinea-pigs and rabbits, in order to study the action that the blastomycetes are able to exercise when they are placed in contact with epithelial and cellular elements, I remarked that when we inoculated the parasites alone they multiplied considerably, and the tissue gave little proliferative reaction, whilst, when we inoculated the parasites together with the solid substratum of nutrition on which they had fed, we obtained a limited multiplication on the part of the cellular elements. I thought then that the different results of the discovery were due to the existence of toxins contained in the solid substratum of nutrition. At that time I began the study of the action which the parasites with the toxins, and the toxins alone, are able to exercise on the common experimental animals. After a long series of researches I succeeded in finding a liquid nutriment in which the blastomycetes isolated from the malignant tumours produce very active toxin. The toxin alone inoculated into the animals gave place to malignant tumours in chemical composition and in structure like those which we remark in man.

With the discovery of toxins we have explained the genesis of malignant tumours. The parasite exists on the skin, on the surface of the mucous membrane, in the little glands annexed to the skin of the normal individual. When, for reasons not yet known, there is a diminution of the organic resistance of the cellular elements which maintain in a state of latent life the microbes which we have in us, these may multiply and give place to the production of the toxins which stimulate to atypical proliferation that group of cellular elements which one finds around the parasites which have multiplied. In this manner the epithelial or connective tissue tumour begins from a limited number of cells. The parasites operate in the first moment of the evolution of the tumour; then the cells altered by the toxins multiply themselves, giving place to the formation of the tumour. When the parasites have finished their vital course they die, and their dead bodies provoke the reaction of the organism, which manifests itself with the formation of an antibody, transforming the blastomycetes into little fuchsinophile bodies.

After having confirmed the existence of the toxin in the culture of the pathogenic blastomycetes, it was interesting to see when they were inoculated into the animals in considerable dose, and for a long time they had the property of determining in the blood of the animals the formation of specific antibodies.

As animal producers of serum, we chose large dogs. On the first days we inoculated subcutaneous connective tissue with the attenuated toxin and then integral toxin, and we saw, after one or two weeks, the antitoxin began to increase to a maximum, which was gained after the third month. The antidote serum may be employed with efficacy in the malignant tumours of dogs and mice, also when $\frac{1}{10}$ c.cm. can neutralize ten times the dose, certainly deadly, of culture containing living parasites and toxin, if, however, the cure does not begin too late. The therapeutic serum, in order to succeed, must be applied early.

It was necessary at this point to study the action of the serum drawn from the inoculated dogs on dogs which presented spontaneous malignant tumours. Dogs, as one says, are subject to contract cancers and sarcoma resembling in course and structure those of man. The theorists have till now considered the malignant tumours as a pathologic process by itself, independent of the organism which brings it. Nothing is more inexact. Malignant tumours, like all pathological processes, provoke, from the part of the organism, a reaction which manifests itself by the production of specific antibodies, as is proved by the slow course of some tumours, the deficient propagation at a distance, the extended regressive metamorphosis in some cases.

When, instead, the reaction is weak, then the tumour develops rapidly; it filters quickly through the healthy tissues, and in the space of a few months the patient dies. All depends on the greater or smaller, slow or swift, active or weak formation of the specific antibodies.

The therapeutic method of the serum consists in

increasing this reaction that naturally exists, but finally, if it is not strengthened by artificial means, it is reduced to a struggle of unequal strength, and the subject generally succumbs. The results obtained prove that this increase of the reparative reaction is possible. In the fourteen dogs affected by cancers of the breast, cancer under the skin, lymph sarcomata, preputial and vaginal, we had complete recovery, without exception, in five or six months. In the dogs in which the tumours had given rise to metastasis—that is to say, it was itself prepared at distance from the original tumour—we remarked that the metastasis disappeared before the principal tumour, and this remark leads us to suppose that the serum operates at first upon the young tumours and after upon those of ancient date. The toxin which represents the stimulus to the proliferation of the cells of new formation, having been destroyed by means of the curative serum, becomes stronger and regresses. From the results obtained up to this we can conclude that treatment by the serum gives the same results in man as in animals. It is a question of pathologic process produced by the same morbific agent, and for that reason they must equally feel the curative action of the serum.

THE MICROBE OF WHOOPING-COUGH.

By Professor Bordet,
Brussels.

The microbe of whooping-cough was discovered three years ago, in the course of certain researches which I undertook in collaboration with Dr. Gengou. The discovery is thus a recent one, and the question arises how the etiological factor has so far escaped the notice of the numerous bacteriologists who have devoted their attention to this subject. The microbe of this disease, although not large, is easily visible, and though somewhat dainty, it grows readily enough on special media. The fact that the problem of the cause of whooping-cough has not been hitherto solved, may be due to the unequal distribution of the germ in the respiratory tract, and the slowness with which it grows in the early cultures. The secretion of mucus in the case of children suffering from whooping-cough is usually abundant, and comes from various points in the respiratory tract. If one examines mucus from the nose and throat one finds usually numerous bacteria of secondary infections, such as pneumococci, influenza bacilli, etc., but the specific germ in this situation is rare. It grows by choice deep in the remoter parts, seldom above the level of the larynx, oftener below it. Further it is at the beginning of the whooping period that the germ is most abundant; later it is much more scanty. It follows from the above that to obtain a suitable material from which to take cultures, it is essential to pick out from the phlegm a strip of viscid exudate, rich in leucocytes, coming from the depth of the bronchi, the expulsion of which marks the end of one of the violent fits of whooping characteristic of the disease in this stage. Naturally, the child must be suffering from whooping-cough alone, not whooping-cough complicated by any other disease.

When the favourable conditions above stated are realized the shreds of mucus are seen to contain numerous leucocytes and a vast number of a small ovoid microbe having the following characteristics: it is non-motile, negative to Gram, staining feebly with the usual basic dyes, such as methylin or toluidin blue; this bacterium is the real cause of whooping-cough. At this point the second difficulty to which I alluded presents itself; even on the most favourable media the germ, at least in the first cultures, grows remarkably slowly, two or three days elapsing before the colonies appear. Again, the exudate must contain the specific germ in almost pure culture, otherwise it is at once outgrown by extraneous organisms, such as the influenza bacillus and pneumococcus. If, however, at the end of twenty-four hours, a fair number of extraneous colonies appear on the plate, the specific germ may yet be separated by stroking with the platinum wire the bare surface of the medium, between the colonies; care must be taken not to touch any visible colony. Fresh tubes are then sown with the wire and incubated. It was by this method that we first isolated the specific germ in pure culture.

The best medium is that described by us in our first paper, and consists of defibrinated blood (human or rabbit) thoroughly mixed with an equal quantity of 3 per cent agar containing a little extract of potato and glycerine. This must be melted previous to mixing. The medium is then set aside to cool.

As regards the morphology of the bacterium of whooping-cough, I have already said that it is a small cocco-bacillus, its size and shape being comparable to those of the bacillus of influenza. Examined *in situ*, it is usually rather longer and plumper, but during the process of subcultivation it becomes smaller and smaller, until it finally appears as a mere point, even under the highest powers.

I will touch very briefly upon its cultural characteristics.

On the blood-agar medium described above it shows a fairly thick whitish streak, the subjacent blood being haemolyzed, but not blackened; this appearance is entirely different to that of other bacteria morphologically similar, such as the influenza bacillus.

The bacterium grows well in liquid media, such as serum bouillon, or blood bouillon, provided that shallow vessels be used, so as to offer a large surface in contact with the air, the bacterium being an obligatory aërobe.

It was our idea at first that the bacterium would not grow at all on the ordinary media, sterilized in the autoclave—for example, agar-agar.

It is possible, however—as confirmed also by Mr. Klimenko—to so adapt the bacterium that it will grow readily on the stock media of the laboratory. If an ordinary agar tube be sown richly with a blood agar culture, no growth appears during the first week. After twelve days the process of acclimatization begins, and a feeble growth shows faintly.

Later, the smear becomes denser, and a new race is born, capable of vigorous reproduction.

A somewhat shorter way of arriving at the same result is to make subcultures into a series of agar tubes, containing a progressively smaller quantity of defibrinated blood.

I propose to come back later to this question of the adaptation of the germ to unfavourable media, and to show you that the race so modified has developed new traits, and that it displays differences of a striking type from the mother culture. I will not enter further into the cultural characteristics; far more to the point is it to inquire into the proofs on which we base our claim that this microbe is really the determining cause of whooping-cough. I have already described how, in microscopic preparations from the exudate, taken at the beginning of the disease, the bacterium is present in enormous numbers and in practically pure culture; this in itself is merely suggestive, but that which to my mind is quite conclusive is the serum reaction of convalescent cases. It is true that it does not agglutinate very strongly; but in relation to the micro-organism which we have described it possesses specific sensitizing properties well defined and invariable.

The method by which this sensitizing power of a serum may be evidenced is too well known for me to describe it in detail here. I refer to the one published by Gengou and myself, under the title of *The Fixation of the Alexine*, a method to which we were led by my earlier studies on immunity, and especially by those connected with anti-microbic and haemolytic serums.

As you know, it was shown in 1895 that an immune serum, such as cholera serum, agglutinates specifically the bacteria with which it is prepared. This principle was extended in the following year by Gruber and Durham to other microbes. Further, cholera serum can induce *in vitro*, without the help of any cell, or the intervention of any vital influence, bacteriolysis of the cholera vibrio. This action is due to the joint efforts of two substances—1, the alexines common to all normal serums; 2, a specific substance, to which I gave the name of "sensibilisatrice," or "sensitizer." The haemolytic serums which I prepared in 1898, by immunizing animals against injections of alien blood, showed that they, too—just as the bacteriolytic serums—owed their solvent power to the combined action of alexine and sensitizer. In all these actions the sensitizer acts as a mordant, by which the alexine is enabled to combine with and destroy the blood or germ cell.

Upon this phenomenon of the "fixation of the alexine" we founded a method which for some time past we have applied to the study of divers infections—typhoid, tuberculosis, plague, etc.—and which has recently been

adapted by Wassermann and his collaborators to the diagnosis of syphilis. Now, in the case of whooping-cough, this method shows in the most precise manner that convalescent children acquire an active immunity against the bacterium under discussion. This is a fact of the greatest value in favour of the claim made for this germ as the determining cause of whooping-cough.

If we admit this claim, the next step is, by means of a more rigorous study of the germ, to attempt to formulate an exact idea as to the solution of the disease.

Whooping-cough belongs to the same group of complaints as diphtheria, in so far as it is a localized infection. The microbe grows in a definite area, but, like the bacillus of diphtheria, it hardly spreads at all, and particularly it never reaches the blood.

What is the cause of the intense irritation of the mucosa—an irritation which provokes these violent paroxysms of whooping? To answer this question, we must study the pathogenic power of the germ.

The ordinary laboratory animals seem to be but little, if at all, affected by the infection of this microbe. An exception must be made in the case of monkeys, as certain workers—Klimenko and Fraenkel—claim to have produced a real whooping-cough by infecting these animals with our bacterium. It is true that Klimenko thought that he had produced the same effect on puppies, but a good deal of doubt has been thrown upon his results. At all events, in the case of the rabbit, guinea-pig, etc., the bacterium is unable to invade the organism when injected under the skin or into the peritoneum.

When, however, these animals are given a few milligrams of culture from solid media into the peritoneum, they die with all the symptoms of profound intoxication, without the development of a real infection. We may, therefore, conclude that the bacterium of whooping-cough is endowed with toxin-producing properties of a high order. By making use of the method introduced by Besredka, we have, in fact, succeeded in extracting an endo-toxin—a poison in the guise of a slightly opalescent liquid. In small doses, which represent only a most minute quantity of microbic matter, a fatal result ensues upon its injection into a guinea-pig's peritoneum, with the production of the same haemorrhagic and exudative lesions which follow the injection of the living germ. Inoculated under the skin in the dose of 0·25 c.cm., it induces a haemorrhagic oedema followed by a widespread necrosis—the skin becoming black—and upon the separation of the slough it leaves a vast ulcer of several square centimetres.

An analogous necrosing influence has been seen clearly on the surface of the bronchi in affected children, as we learn from interesting data furnished by Leonardo Domenici. If, from the bronchus of a child, a small quantity of whooping-cough exudate, containing the specific germ in large numbers and pure culture, be injected into the anterior chamber of a rabbit's eye, no infection takes place, and no microbic growth results. In the course of a few hours, however, there ensues a necrosis of the cornea, which becomes white and opaque, as though it had been cauterized, coincident with a very acute conjunctivitis and extraordinarily profuse lacrymation. We may, therefore, admit that the persistence of the whooping-cough in children can be ascribed to the existence of lesions in the cellular lining of the bronchi due to the action of a remarkably irritant toxin. This would explain, too, why the whooping ceases to be a feature of the clinical picture during the period immediately preceding convalescence, when the germ has become very rare in the exudate.

The child has now succeeded in getting rid of the virulent organism, but repair of the damage caused by the poison takes place but slowly. Precisely for this reason, too, serum therapy has failed to give the results which were hoped for. The diagnosis of whooping-cough is nearly always delayed until the appearance of the characteristic whoop; by this time the infection has obtained a firm hold, and the toxin has already produced its necrotic effect.

I think that vaccination by killed cultures should be much more useful; but the difficulty of introducing into general acceptance such a prophylactic is enhanced by the fact that, in the majority of cases, the disease itself seldom assumes a very serious type.

Such, gentlemen, are the principal facts relative to the etiology of whooping-cough and to the interpretation of its more obvious symptoms. They have already been published for some time, but with your permission I propose to conclude by bringing to your notice, very briefly, certain facts which we have observed quite recently, and which have not been hitherto made public. They will, perhaps, interest those among you who are concerned particularly with the problem of immunity. I have been able to confirm them during the execution of a research undertaken in our institute in collaboration with Dr. Heeswijk, of Amsterdam.

The problem which really seems to be the must puzzling in the study of immunity, and perhaps also the most important, is the question of the specificity of the prophylactic serums obtained by immunization.

We are able, thanks to these serums, to diagnose the nature of bacteria, and to recognize the origin of real blood cells or albuminoid substances. The precision and power of selection shown by the organism in elaborating substances specifically correlated to the vaccine are really marvellous. Now, one of the questions which present themselves in regard to this specificity is evidently as follows. We are able, thanks to these agglutinating serums, to recognize a given microbe and to differentiate it from others. But this characteristic which a microbe possesses of reacting with an appropriate serum, is it inalienable? May it not, perhaps, disappear when the germ is subjected to certain conditions of existence? In particular reference to this point, how far does it depend upon the nature of its food supply? As long ago as 1896 it was noticed by Metchnikoff and myself that the cholera vibrio, as the result of being subjected to certain vicissitudes, such as prolonged contact with leucocytes, acquired a distinct resistance to the agglutinating influence of cholera serum. Analogous examples of this phenomenon have been published since, and the researches of Grasberge and Schattenfrol on quarter-evil are very instructive in this respect.

Quite recently Dr. Heeswijk and I have found that the bacterium of whooping-cough, when it is subjected to special conditions of existence, may lose certain of its characteristic elements, especially the antigen which reacts to the agglutinin of the specific serum. There is thus an actual loss of the distinctive signs which renders possible the sero-diagnosis.

I mentioned a little while ago that the microbe of whooping-cough prefers a blood medium, but that it can be accustomed to live on ordinary agar. We thus get two distinct varieties, which we may call the blood germ and the agar germ.

In relation to the serum reaction, these two varieties behave quite differently.

If on the one hand an animal be immunized against the blood germ, and on the other hand against the agar germ, serums by no means identical are obtained.

Lack of time forbids me to enter into details. I will content myself with quoting one experiment. Let us take the case of the serum of a horse immunized against the blood germ. This serum agglutinates the latter microbe up to a very high standard, whereas it is infinitely less active towards the agar germ.

Moreover, it can be shown that the reason why the blood microbe reacts so strongly towards this agglutinating serum is that it contains a definite chemical radical—an antigen, to use the common term—which is entirely absent from the agar germ.

A large dose of a thick emulsion—in physiological saline—of the agar germ is mixed with a minute quantity of the above-mentioned serum; a few hours later this is centrifugalized and the supernatant fluid decanted.

Even after this prolonged contact the fluid will be found to have retained intact its original agglutinating standard towards the blood germ. On the other hand, control tests made by mixing the blood germ with the same serum show, as is to be expected, complete absorption of the agglutinin.

We may therefore conclude that the microbe of whooping-cough when grown on common agar loses the property of reacting with an agglutinin capable of influencing the same germ when grown on a medium rich in blood. Further, this property is regained when the agar germ is replanted in blood media.

With regard to the agglutination reaction, the bacterium behaves as if it belonged to two different species, depending upon the nature of the medium in which it be grown.

Thus it may be considered that specific antibodies such as agglutinins are aimed not so much at the essential constituents of the bacilli as at products of secondary rank, which may or may not be formed and which are non-essential, and do not remain unchanging through succeeding generations.

DISCUSSION.

Dr. J. FREEMAN (London) said: We are all of us fortunate in the opportunity of listening to Professor Bordet on this subject of whooping-cough, and I particularly so, because in April, 1908, I received from him a culture of Bordet's bacillus, and I have been working at the subject ever since.

In the paper which has just been read M. Bordet has refrained from dwelling upon the unusual difficulties of his task and upon the extreme refinement of method and technique which it demanded. The microbe grows only on special media, and then very slowly, there being a period of three days before the first colonies are visible; and, in addition, the microbe, when isolated from the expectorate, was indistinguishable from the influenza-like bacillus common to 80 per cent. of whooping-cough cases, to throat diseases, and normal throats. Time will not allow me to say anything now about the possible importance of this influenza-like microbe, but without doubt this similarity greatly increased the difficulty of isolating Bordet's bacillus.

These two microbes, which cannot be distinguished in the exudate, are alike in being short, slight bacilli, not staining well, but staining best with weak fuchsin. Neither retains Gram's stain. In artificial culture they are distinct enough; microscopically, influenza tends to be pleomorphic, Bordet, to be more regular; influenza will only grow in the presence of haemoglobin; Bordet will grow abundantly on serum agar; influenza reaches its full growth in twenty-four hours, Bordet, when examined with a lens, rarely shows a trace of growth within the first twenty-four hours; influenza never grows beyond the minute, scarcely visible, dewdrop-like colonies, Bordet will, after forty-eight hours, show a growth as luxuriant as staphylococcus.

As to the specificity of Bordet's bacillus, the conclusions of Drs. Bordet and Gengou, which are based chiefly on the serum reactions of whooping-cough patients, have been confirmed by workers all over the world. I personally was convinced when I found that cultures obtained from Professor Bordet in Brussels were specifically agglutinated by serum of children with whooping-cough in London and would deflect the complement with such serum. The agglutinative reaction is not very strong—complete agglutination in sixty-fold dilution of serum was about the highest found—and it does not always take place. Failures often seemed to be in consequence of differences in the bacterial emulsion, such differences as the age of the culture or the medium on which it was grown. On the other hand, agglutination was not obtained above eight-fold, and was seldom obtained above four-fold dilution in the case of a large number of non-pertussis serums.

In connexion with the statement by Professor Bordet that the immunity to the agar grown variety differs from the immunity to the blood-agar grown variety of his bacillus, I can confirm this by my own experience. After growing the bacillus on both serum agar and blood medium for some time I left off the blood medium because the serum agar was easier to make, and for three months I failed to find any trace of agglutination in serum from pertussis patients. I then learnt through Professor Bordet that if I transferred my cultures to blood medium the agglutinating power would return, and I found this to be so.

There is one more confirmation of the specificity of Bordet's bacillus which will be very convincing to those who have worked at vaccine therapy. Bordet gave prophylactic doses of sterilized bacterial vaccine to some twenty children who were in contact with infected cases. These children shortly after caught whooping-cough and caught it severely, so severely that it was impossible to persuade the mothers that a living culture had not been inoculated. This deplorable result is of value to us because it seems clear that the children got a profound negative phase after the dose and in this state they fell victims to the infection.

These facts point to the specificity of Bordet's bacillus, but it is difficult to make the serum tests quantitatively for the purpose of controlling therapeutic inoculations of the organism. The deflection reaction of Bordet and Gengou has been hitherto qualitative only. The agglutination of the microbe seemed too irregular in its manifestation to give a constant quantitative result, and therefore one turned to the opsonic index as being by nature a quantitative method; but here the fact that the bacillus stains badly proved a terrible obstacle. I have not yet obtained with Bordet or influenza an opsonic film which I felt I could trust. I was constantly coming across leucocytes concerning which it was, after an examination lasting ten minutes, impossible to say if they should be credited with ingesting five or twenty-five bacilli. This difficulty must be due to staining defects or digestion by either leucocytes or serum, or to faults in the bacterial emulsion, and I still hope by investigating these points to obtain trustworthy indices to Bordet.

Having thus failed to get what I wanted from (*a*) the Bordet-Gengou reaction—that is, deflection of the complement, (*b*) agglutinative reaction, and (*c*) the opsonic index, I tried what I could do without the guidance of a serum reaction, and fell back on observation of the clinical result. For this purpose I gave upwards of 1,000 test inoculations of a vaccine prepared from Bordet's bacillus to children with whooping-cough. In order that the children might not be deprived of the advantages of orthodox treatment, the placebos, expectorants, and aperients were administered by another hand.

The following precautions were taken:

1. Every alternative case was kept as a control, and was inoculated with physiological salt solution and not with vaccine, and the mothers or nurses who made the reports as to the coughing of the child, etc., were not told that half the cases were control cases.

2. Each report on a vaccinated or control case was placed by me in one of five classes: Much better, better, unchanged, worse, and much worse.

3. Another precaution was taken after the experience of the first month, the work of which had to be discarded. I took the reports in ignorance of whether the child had been inoculated with vaccine, with the result that I found that during the first month I had been unconsciously biassed in favour of the vaccine cases.

The doses began with 5 million, sank to 2½ million, and afterwards rose gradually to 20 million. To summarize my statistics, the percentages were:

Class.	Vaccine Cases.	Control Cases.
Much better	31.0	21.55
Better	37.1	34.55
Unchanged	15.7	23.9
Worse	15.2	18.15
Much worse	1.0	1.85

These statistics show a steady advantage of the vaccinated cases over the control cases, and the experiment showed that a larger dose gave a greater advantage than a small one.

Two conclusions are suggested to me by my experiment:

1. Before any judgement can be formed by noting results of treatment, close observation for a long time, and with many precautions, is needed. More progress and more certainty would be attained by six days' work in a laboratory than by a year in the wards. When a doctor reports that he has a drug which he has proved to be of distinct benefit, he should be asked, In how many cases have you tried it? Did you have a system of controls? and Did you have a colleague to report who did not know which of the cases were controls?

2. The vaccine of Bordet's bacillus is beneficial in cases of whooping-cough, but the optimum dose has not been ascertained. Possibly better results may be obtained from larger doses, but finality will not be assured until a trustworthy serum reaction has been obtained.

SECTION OF DISEASES OF CHILDREN.

Harold J. Stiles, M.B., F.R.C.S.Edin, President.

DISCUSSION ON THE TREATMENT OF CLUB-FOOT.

OPENING PAPER.

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Although the term "club-foot" is used to denote nearly all distortions of the foot, whether acquired or otherwise, it has been wisely decided to exclude from discussion the paralytic type. Of the congenital varieties, equino-varus is the only one of serious clinical import. Pure varus and valgus are extremely rare, and the treatment of talipes calcaneo-valgus is of the simplest.

Talipes Equino-varus.

The primary distortion of equino-varus is a dislocation inwards of the anterior part of the foot at the medio-tarsal joint. It varies largely in the same case during the process of growth, and it varies according to the severity of the deformity. There are certain elements, however, almost constant. The scaphoid articulates with the side of the head of the astragalus instead of with its anterior surface; it also lies more towards the under side of the astragalus, and thus leaves the astragaloid head abnormally uncovered. The cuneiforms and metatarsals follow the direction of the scaphoid. The cuboid is displaced upwards and inwards, and may be generally found lying to the inner side of the anterior part of the os calcis. The astragalus later is depressed anteriorly and more wedge-shaped than normal. It is plantar flexed, so that much of its body lies anteriorly. Its neck is generally longer than usual; it is depressed and displaced inwards. The os calcis is in the position of plantar flexion, and it lies beneath the astragalus, instead of to its outer side. In a large proportion of cases there is an inward rotation of the tibia and fibula, so that the external malleolus presents in front of the tibia. This last is an important point to remember, and I shall refer to its clinical bearings later. The ligaments and muscles on the concave aspect are shortened and resistant, the adductor and flexor group being relatively stronger than their lengthened and overstretched opponents. In the less advanced cases the most obstructive structures are the plantar fascia and the ligaments which bind together the os calcis, astragalus, and internal malleolus. The muscles which are shortened and tend to increase the deformity are the tibialis anticus, posticus, the gastrocnemius, and the soleus. These changes, as they exist in a case of recent talipes, may be comparatively slight, but later on, as the foot develops, and more especially when walking is commenced, the bones participate with the increasing deformity in making fixed and inflexible the inturned foot.

In consideration of the short time at my disposal, I may be pardoned if I state my own procedures as regards treatment rather than discuss the methods of others.

Age for Treatment.

Believing, as I do, that with appropriate early treatment grave operative procedures would never be needed, I shall largely confine my remarks to the treatment of the early case. We, of course, constantly meet cases where it is expedient to attack bone. Such cases, however, involve neglect on the part of the parents or insufficient treatment on the part of the practitioner. If one has been given a free hand in the treatment of an early talipes, there is no manner of excuse for failure, and the so-called relapsed or intractable case should never result.

Two important questions greet us at the onset—When shall treatment start, and when shall treatment end? The treatment should begin at birth, and should not end until the little patient can voluntarily hold his foot everted and ankle flexed. Treatment should begin at once, for there is no class of case which more bitterly resents procrastination or insufficient handling. We must be taught to realize the responsibility which rests on any one who postpones the immediate treatment of club-foot. The blame cannot be thrown on the midwife, for invariably she calls medical attention to the defect. To any surgeon of practical experience of this deformity it is not necessary to state that a baby of 1 day old is an infinitely easier problem than one a month old, and that very often the treatment of a child at 3 months may require very skilled and forcible effort. I emphasize this the more as, even yet, we have authors who mischievously write of 6 weeks and 10 months as a suitable age to begin treatment. I have never seen a case of club-foot where treatment has been started in the first week where the deformity could not be completely rectified by manipulation and retention in two months. Such cases have not voluntary power in the overstretched muscles for a much longer time. This is due, however, to physiological reasons in connexion with the elongated tendons, and not to rigidity of the foot.

Technique.

It is customary for the textbooks on orthopaedic surgery to allude to four or five classes of cases. The first includes the cases which can, by very mild manipulation, be easily rectified; the last would comprise the adolescent, untreated, so-called intractable case. This merely means that cases are met with in every stage, and obviously must be overcome by various and diverse methods. It is to be remarked, however, that one child a day old may present much more formidable deformity than another of the same age, but we should at once be able to predict an easy or a difficult course.

The simple case is the one which we can quite easily replace in good position, and which shows but little adduction at the mid-tarsal joint. Such a case may not require a division of even the tendo Achillis. If, however, there is a marked adduction at the mid-tarsal joint and a rotation inwards of the tibia and fibula, considerably more difficulty will be experienced.

In the simple case the tibia and fibula should be grasped at the epiphysis, and the foot should be alternately everted and flexed. In all manipulations for club-foot the epiphysis should be grasped tightly to avoid strain. Following eversion and flexion the heel should be grasped in one hand and the anterior portion of the foot in the other, and abduction practised at the medio-tarsal joint. Five minutes spent three times a day in doing this should be followed by gentle massage of the flexors and peronei. Then comes the application of a bandage or of a rectangular splint. Few seem to appreciate how much can be done by the mere application of a calico bandage. To invert a foot the bandage would be started on the outside; to evert, on the inside. In the case of equino-varus it should start on the inner side of the ankle, pass under the sole of the foot, over the front of the ankle, and so evert it. Every turn of the bandage so applied pulls the foot outwards. I have on several occasions, for demonstration purposes in cases of moderate severity, completely overcome pronounced deformity by the simple use of a bandage. Care should be taken if a bandage be used, with or without a splint, not to carry it much above the ankle, lest the muscles be thereby weakened. In these early simple cases, when frequent manipulation is employed without retention, an intelligent nurse can be easily taught to lighten the labours of the surgeon.

Should the surgeon be able with but little effort to restore the foot to above a right angle, division of the tendo Achillis will not be needed. Should the degree of resistance be more marked, division is to be preferred. In such cases I correct the varus and the equinus at one sitting. I begin by placing the foot in a sterile wrap and lever it at the mid-tarsal, in obstinate cases using a block to increase the force. When the adduction is completely over-corrected I divide the Achilles tendon and fix the foot in a retention splint.

A good deal has been written of the advantage of correcting the varus, and at a later date attacking the tendon. Although in full possession of the arguments for this view, I do not subscribe to them, and I know from abundant observation that the tendo Achillis becomes a powerful invertor of the foot. This is due to the altered position of the os calcis, and if you examine a case of club foot you will generally find the Achilles tendon displaced inwards, the extent of displacement being proportionate to the severity

of the inversion of the foot. This factor of inversion seems to have been quite overlooked, and yet it has a very important clinical bearing. So marked is this displacement in certain advanced cases, that I have divided the tendo Achillis longitudinally, and separated one-half of its insertion, passing it under the outer half of the tendon and affixing it to the outer half of the heel.

For a simple division of the tendon the seat of election should be well above the insertion to avoid the fan-like expansion. The division of the tendon is often done in very bungling fashion. It is better to discard the dangerous old-fashioned knives which even yet adorn our text-books, and to use small-bladed tenotomes. The child should be made to lie on its face, while a nurse flexes the good limb well out of danger. The position of the tendon can be felt with the handle of the knife, and an indentation made with it in the skin to denote its position. The surgeon holds the foot in an extended position with the left hand, and enters his knife to the inner or outer side of the tendon denoted by the indentation made in the skin. The foot is then slackened while the blade is turned beneath the tendon, then extended so that by a short sawing movement the tendon is divided upwards. This short sawing movement is an effective safeguard against dividing the skin, an accident very common in the hands of a beginner.

Fig. 1.—Club-foot shoe.

Fig. 2.—Club-foot shoe.

At this point another problem arises. Having divided the tendon, should the foot be at once placed at right angles to the body? There is no object to be gained, only valuable time lost, by the gradual separation of the ruptured tendon. The fear that union may not take place is groundless. I have divided the tendo Achillis many thousands of times, and only once do I remember a failure of union. The foot should be forcibly flexed by means of the hand or wrench, and at this stage we should satisfy ourselves as to the presence or otherwise of rotation inwards of the tibia and fibula.

We must all have noticed that in many cases where flexion and inversion have been corrected there seems to be a rotation inwards of the leg when the patients walk. There has been much discussion as to the site and cause of this rotation and many theories have been promulgated, but I am convinced after examining many cases that, with rare exceptions, it occurs entirely below the knee. In old cases, where this rotation complicated the result, I advocated osteoclasis for its correction as a substitute for girdle bands and other complicated and hateful machinery. I would urge, however, that this calamitous complication be obviated at the outset. From the moment treatment is started the malleoli should be grasped in one hand, while the leg should be held below the knee with the other. The lower hand rotates outwards and the upper inwards. The leg is thus twisted in its long axis by an action not dissimilar to that used when one wrings a wet cloth. If this be done each day, by the time the foot is straight the rotation of the leg will also have disappeared.

In cases which come to the surgeon for the first time at 3 or 4 months old, other operative procedures may be needed in the form of subcutaneous division of any tightened soft structures. This may include the plantar fascia, the two tibial tendons, the anterior fibres of the deltoid, and any deep plantar ligaments. The anatomy must not deter the practitioner who is outside the call of help. It is far better to divide any of the tendons, with the artery as well, than seriously to delay treatment. In my long experience I have divided many structures best avoided when performing tenotomies, but no appreciable harm has ever followed.

In this, the more severe type of case, ranging from 3 months to 4 years, I supplement manipulation by the use of a Thomas wrench. Thomas himself very seldom tenotomized more than the tendo Achillis, and he effected the restoration of the foot by intermittent stretching and retention in an iron shoe. This method is applicable to any degree of deformity in young children, and the ultimate results are excellent. There is much to be learned as to the use of the wrench. The twisting and bending is done forcibly and quickly, and the foot immediately released, lest the skin be injured. The wrench should tightly grip the foot so that it does not slip and

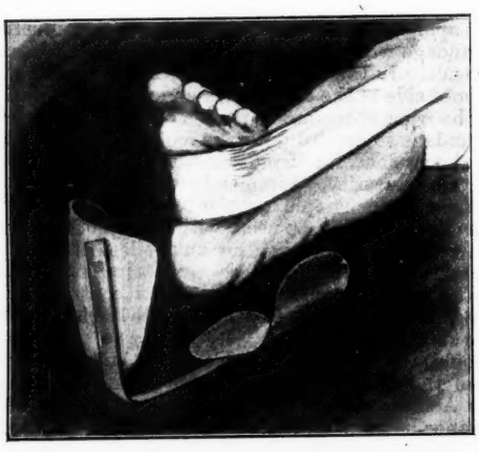


Fig. 3.—Retention splint.

Fig. 4.—A simple way of retaining foot in overcorrected position.

make a sore. In older children the surgeon's hands should grip the epiphysis while the wrench is worked by the hip. In this way a steady and easily controlled force is applied. This moulding should be conducted every day for three weeks, and each time should be carried far enough temporarily to destroy the resiliency of the soft parts. I use a club-foot shoe, modified from that used by Thomas, in order to produce better eversion (Figs. 1 and 2). The details of its application, simple as they appear, require study. After the foot has been manipulated into the best possible position, a piece of plaster should be started on the dorsum to evert the foot and given to an assistant to hold. He should be directed to pull at right angles to the extended leg, whilst the surgeon, or one he can trust, places the retention splint in position (Fig. 3); this should be done every day for six weeks. I much prefer this method to plaster-of-Paris, although in the case of hospital patients the less troublesome fixation by plaster is inevitable.

At the end of two months the most troublesome cases of this class—that is, cases which the surgeon sees during the first two or three years—should present a pliable foot, which can be placed into normal position without encountering resistance, and if the child be sufficiently old he can be taught to walk in such a manner that each step he takes tends to improve the shape of his foot.

No case of talipes equino-varus can be pronounced cured until the patient has tested it by walking. Surgeons think that, as soon as they are able without effort to overcorrect deformity, the case is cured. This is a most serious fallacy. May I impress upon you, for textbooks do not state it, that no case of club-foot is cured until *the patient can voluntarily raise his own foot* from the *deformed into the corrected position*!

Causes of Relapse.

There are three causes of so-called relapse in club-foot:

1. Insufficient correction of deformity.
2. Erroneous deflection of body-weight on tarsus when walking.
3. A slack and lengthened condition of muscles due to over-stretching.

If the deformity be not sufficiently corrected in the first instance, the old condition of things is sure to return because of the altered shape of bones and structures, more especially the ligaments. Roughly speaking, the change in bones and ligaments on the convex aspect of a talipes has resulted in an increased surface measurement. Unless this is quite obliterated by treatment, the unequal growth goes on until the so-called "recurrence" again taxes our resources.

I have said that no case should be looked upon as cured until walking has been practised; in other words, the foot, when the time for walking comes, will either be made worse or perfected by this exercise. If it be made worse, it is due to the fact that the superincumbent body-weight is placed too much to the outer side of the tarsus, and that therefore every step the patient takes tends to emphasize the varus. One can see how harmfully walking would affect such a case. Walking should give the finishing touches to the surgeon's art, and should not be allowed until the body-weight at every step tends to flatten the plantar arch.

The effect of lengthened muscles opposed to deformity is very obvious, and when it is realized that in every case of club-foot the peronei and flexors of the ankle have for months remained in a stretched condition, while the tibials and calf muscles are short and unstretched, it is easy to understand the imperfect recovery where no attention is paid to the strengthening of the weak group of muscles.

In point of fact the flexors of the ankle and the peronei, which have for so long remained overstretched, behave to all intents as paralysed muscles would, and gravity decides the issue, exactly as it does in the case of an equino-varus due to poliomyelitis. When the patient has arrived at the age for walking and the deformity is corrected, and, most important of all, when he can voluntarily evert and flex his foot, he should be taught to walk. Correct deflection of body-weight on tarsus must, however, be assured. This is best done by a simple device which I show you. It consists of an iron bar, called a flapper, which skirts the outer side of the sole, and the foot, aided by this and an inside iron to which it is bandaged, keeps everted during walking.

By means of this little addition to the boot and the iron on the inner side of the leg, the body-weight is deflected from the outside of the tarsus to the inside. Every step, therefore, the patient takes corrects, howsoever little, any tendency that may remain towards inversion. The nurse should further be instructed to train the child to walk with out-turned toes in order that body-weight be employed in stretching the deltoid ligament on the inner side of the ankle. During the whole course of treatment, massage of the flexors of the ankle and of the peronei, together with resistance exercises, should be practised, and every effort made to render flexible and pliant the mid-tarsal joint.

Operation in Adults.

Although deformity, even to an adult age, can be rectified by forcible means, it is often advisable to resort to bone operations. Every year I operate upon twenty or thirty such cases. I have no time even to name the many operative procedures described, but I should like to dwell upon a certain point which should be observed before any operation involving the removal of bone be performed. It is this—that a preliminary and thorough stretching following tenotomies should precede the more radical operation.

This is a matter of considerable importance. Before removing bone in a patient of any age, the foot should be placed in the very best position by means of wrench and tenotomy, and retained there in splints for a week or more. The operative result in consequence of this preliminary will be much more artistic, and will involve far less sacrifice of bone.

If the bone operation is decided upon, I usually make a longitudinal incision at the outer aspect of the dorsum of the foot, its centre being over the prominent head of the astragalus. The extensor tendons of the toes are retracted to each side of the wound, the mid-tarsal joint is opened up, and a good view of the astragalus and scaphoid is obtained. In some cases I remove a large wedge, the base being to the outer side. This involves the astragalus, cuboid, and part of the scaphoid. Enough must be removed to bring the foot into an overcorrected position, otherwise some return of deformity will take place during growth.

Of late years I have improved upon this procedure, and I now remove most of the astragalus, leaving not much more than the superior articular surface for articulation with the tibia and fibula. In order to do this I scoop out the cancellous tissue with an arthrodesis gouge, remove part of the cuboid and scaphoid as before, and run the chisel through any other portions of bone which prevent easy eversion. By this operation one overcorrects adduction at the mid-tarsal joint, and in this way subsequent growth of the deformity is obviated. No operative routine, however, can be prescribed, and each surgeon after studying his case must adopt the operation he thinks the best.

Conclusions.

1. The treatment of club foot should begin at birth, and should not end until the patient walks.
2. Early treatment should assure a complete recovery without any operation on bone.
3. The mechanical treatment of the rotary deformity of the tibia and fibula should be synchronous with that of the equino-varus.
4. The test of recovery is the ability of voluntary correction of deformity.
5. The so-called relapse is due to inefficient correction, to lengthened extensor tendons, and to erroneous deflection of body-weight during walking.
6. When bone is to be removed a preliminary forcible correction of deformity by tenotomy and wrenching both simplifies and perfects the operation.

DISCUSSION.

Mr. James H. Nicoll (Glasgow) said: In his plea for the earliest possible commencement of treatment, in his condemnation of complicated splints, and in the stress he lays on the importance of making the measure of the success of treatment the degree of *voluntary* correcting movements by the patient, I am thoroughly at one with Mr. Robert Jones, and if I say anything further regarding two points, it is not in the way of disagreement, but rather

by way of extension of what I take to be Mr. Jones's views. These points are:

1. The treatment of the ultimate difficulty in cases of club-foot, the correcting of the "in-toeing" which persists after the club-foot has been cured. With Mr. Jones's view, that the inversion of the limb is entirely below the knee, I quite agree, and that it can be cured by repeated twisting of the leg bones I know from experience. It may be the arrangement of the "heads" of his address which makes it appear that Mr. Jones defers this twisting of the leg till a late stage of the treatment. The general lines of his argument suggest that he and I are of essentially the same practice in the matter. The twisting or wrenching of the leg should be begun at the same time as that of the foot, and persistently carried out in both leg and foot (by the mother or nurse daily, and, if need be, at intervals under general anaesthetics by the surgeon) till cure is obtained.

2. The importance of a judicious decision as to the line of treatment to be followed ere the case is undertaken. Mr. Jones wisely says that the surgeon should carefully study each case and choose the line of treatment which appears to be appropriate to it. Mr. Jones will, I feel sure, agree to this extension of his remark, that the surgeon should devote some of his preliminary study to the *mother* as well as the patient. Given an intelligent mother or nurse, many of even the severe cases of club foot can be cured by manipulation, wrenchings, and plaster-of-Paris; and from that follows this, that our students get from textbooks and what they see in *hospital* practice a badly focussed view of the treatment of club-foot. Personally I regularly employ tarsectomy as routine practice in hospital to obtain an almost absurd degree of over-correction, because I know that that is the child's only chance of cure. Amongst the poor, intelligent after-treatment, or perseverance in manipulative treatment, is not obtainable. Hence students see things done to club-feet in hospital which are quite uncalled for in private practice. Of late I find I am tending to substitute for tarsectomy, in all cases under 1 year of age, the following procedure: Having divided the tendo Achillis, I insert a stout tenotome in the fold on the inner aspect of the sole in front of the malleolus, and (by a combination of tenotomy and syndesmotomy) divide everything within the inner two-thirds of the sole to the bones, and then carry the knife clean through the cartilaginous tarsus, stopping short just under the dorsal tendons. Bleeding is trivial, overcorrection is ample, and the foot is fixed in plaster in the fully overcorrected position.

Mr. ALEX. MACLENNAN (Glasgow) said: In the treatment of some cases I would plead for the Adams principle. He aimed at the correction of the varus before the equinus was dealt with; indeed, the fixation of the heel by the tendo Achillis was utilized to reduce the varus element. That the Achilles tendon has a preponderating effect in the production of the common equino-varus type is proved by the fact that during manipulation the varus and the equinus are found to be vicarious. The muscles acting through the tendon are therefore inverters of the foot, and if we can reduce this action we assist the cure. Instead of performing a tenotomy merely, a lengthening of the tendon permits of the localization of the pull to the outer section of the calcaneum, and thereby the tendo Achillis group of muscles becomes a pronating force. The technique I have employed for some years may possibly be that commonly employed by others, though I am not aware of it. It consists in freeing the tendon through an incision, which starts at the inner side of its insertion and passes obliquely upwards and outwards. The tendon is split longitudinally for an amount corresponding to the amount of lengthening requisite. The inner section is severed at its insertion into the calcaneum, stitched at its free end into the unsevered outer section higher up, then the outer part of the tendon is cut above the attachment. This technique is applicable to the smallest tendon. In some cases, where the remaining equinus has been marked, the reduced intermalleolar space forbids the rolling backwards of the astragalus, and if the ankle is forced to a right angle the foot tends to become displaced bodily forwards. In such conditions malleotomy gives a satisfactory result. In those cases where a cuneiform osteotomy is considered necessary, the removal of a large wedge markedly shortens the foot, and to avoid this I have in some cases reduced the breadth of the wedge, and, instead of removing it, have inverted it, so increasing the effectiveness of the osteotomy. As a slight divergence, instead of making the incision parallel with the peroneus tendon, I make it directly at right angles with it, and corresponding to the wedge of bone to be removed.

Mr. LAMING EVANS (London) said that he proposed to follow Mr. Robert Jones's example, and confine himself to a statement of his own practice.

For this purpose he had examined the notes of all cases of congenital equino-varus that he had treated during the septennial period July, 1902, to June, 1909. The cases numbered 232, of which 183 had been operated upon. A large proportion were severe, and of the so-called "relapsed" variety; 505 operations under general anaesthesia had been necessitated. An analysis of these 505 operations showed that 161 were forcible manipulations, 327 were performed upon tendons and soft structures, 17 only upon bone. In infantile cases the régime laid down by Adams was followed in all cases with but slight modifications. The varus of the anterior tarsus and metatarsus was fully corrected and as much of the subastragaloid supination as possible was rectified before the equinus deformity was dealt with. For this purpose, in the very large majority of the cases, subcutaneous tenotomy of the tibialis anticus and posticus was performed; and whilst the varus was being gradually corrected by manipulations and retention by flannel bandages upon a malleable steel splint the os calcis was rotated outwards under the astragalus by manipulation and by the pressure of the bandages. Not until the varus was completely corrected was any attempt made to correct the equinus; this, in every case, necessitated division of the tendo Achillis. The residual sub-astragaloid supination was then corrected by manipulation at the same that the equine deformity was completely corrected.

The technique employed differed slightly from that advocated by Adams in two particulars: (1) At the time when tenotomy was performed, the child being under a general anaesthetic, further correction of the deformity was obtained by more forcible manipulations than he employed; (2) whilst under the anaesthetic the foot was bandaged to the splint, or retentive apparatus, in as fully corrected position as the parts would allow, due precautions being taken to avoid pressure sores, or interfere unduly with the circulation. After correction, the foot was kept constantly bandaged in a rectangular tin shoe, except during two intervals during the day when it was massaged and gently manipulated. At an age between 18 months and 2 years a boot and walking support were fitted. The support extended on the inner side as far as the knee, and on the outer side from the knee to the hip. The knee-joint was free, and allowed the child to run about freely; a night shoe at this stage was also imperative. The time during which the child wore the walking support varied rather with the assiduity with which the mother carried out the manipulations and exercises than with the severity of the case. The object of the support was to correct the internal torsion of the leg bones and consequent inversion of the foot.

Among children who had walked, his cases included various degrees of deformity between cases in which full correction had not been obtained or maintained and the most severe cases of inveterate or relapsed cases, and his method had varied according to whether the patient was treated as an in-patient or out-patient. His rule in the former case had been division of any contracted tendon or ligament, supplemented by forcible manipulation and subsequent gradual rectification by a modified Scarpa's shoe, preferably by one with five different movements controlled by five different racks. With out-patients a series of plaster-of-Paris dressings were used until the correction was advanced sufficiently to allow the foot being retained in a rectangular tin shoe.

Of the 17 operations which had involved the skeleton, the youngest child operated upon was aged 6. At this age, a subcartilaginous enucleation of the head, neck, and part of the body of the astragalus was performed. In a child of 8, a linear osteotomy through the neck of the astragalus and anterior part of the os calcis was performed.

In intractable cases after the age of 10, he had in general preferred the operation of cuneiform tarsectomy to that of astragalectomy. In this operation the cuboid, the anterior end of the os calcis, the head and neck of the astragalus, and perhaps the scaphoid, had had to be removed before full correction had been obtained. He aimed at slight overcorrection, as he had found that any subsequent manipulation on an incompletely corrected foot after tarsectomy added little or nothing to the result. He had found that a complete astragalectomy could sometimes be avoided by a removal of the head and neck of that bone and a subperiosteal removal of the cancellous bone of the body. This allowed a freer subsequent movement at the ankle joint than after a complete removal. If the residual deformity—for which a bone operation was necessary—consisted mainly of subastragaloid supination with equinus, a pure astragalectomy had given the best results. His rule had been to try and correct as many cases as possible without interfering with the skeleton, for by this means a more elastic and supple foot had been obtained; but, if a bone operation was imperative, a free removal had been undertaken to obtain a slightly overcorrected position of the foot.

Mr. Edmund Owen (London) thought that it would have been better if the discussion could have been kept in three separate grooves: The treatment of early cases of congenital club-foot, of later or relapsed cases, and of paralytic cases. It was impossible to begin the treatment of congenital club-foot too early. In not a few cases of equino-varus division of the tendon of Achilles sufficed to correct the inversion of the foot as well as the drawing up of the heel. In worse cases various other tendons had to be divided, and that might be best effected by very free use of the scalpel in a subcutaneous operation. Every resisting structure must be cut, ligaments as well as tendons. The late Sir T. Fitzgerald of Melbourne used freely to divide all resisting structures by using a chisel passed in from the dorsum; with that instrument he practically minced up the bones. Mr. Owen also spoke in high terms of the late Mr. Phelps's open operation for relapsed or inveterate cases.

Dr. F. W. Collinson (Preston) suggested that in serious cases external tarsectomy was less severe than Phelps's operation, and more likely to be successful. He had found frequent wrenching under chloroform with the use of plaster-of-Paris most successful, but necessitating very careful watching afterwards.

The President (Mr. Stiles) said he felt sure he expressed the feeling of the Section when he said how greatly he felt indebted to Mr. Jones for his admirable paper, and for the very clear and practical manner in which he had given the methods he himself was in the habit of employing. He acquiesced in all that Mr. Jones had said with regard to the manipulation treatment in infancy. The difficulty, however, was to get this treatment adequately carried out in hospital patients. He agreed with Mr. Owen that in mild cases it often sufficed simply to divide the tendo Achillis. From what he could gather from the gentlemen who had already spoken, it was evident that he (Mr. Stiles) practised tarsectomy more frequently and at an earlier age than the majority of surgeons. The reason for this was that he was not quite so expert in the use of the wrench as Mr. Jones. In an aggravated case of club-foot in a hospital patient 1 year or more old, his practice was to divide the tendo Achillis and then proceed to tarsectomy. He preferred to divide the tendo Achillis by the open method, because in addition to dividing the tendon itself, it was necessary also to divide the whole of the sheath and the fascia over the deep flexors. After this the foot was forcibly dorsiflexed in order to stretch the posterior ligament and the deep flexors. In removing the wedge of bone from the tarsus he preferred the transverse incision recommended by Kocher. The wedge of bone removed comprised the anterior process of the os calcis, the cuboid, and the head and neck of the astragalus. In some cases it was necessary to extend the wedge right across the foot so as to include the scaphoid. It was essential to remove enough bone to enable the foot to be overcorrected. He quite agreed with Mr. Tubby as to the necessity of removing enough of the astragalus. In bad relapsed cases, and in aggravated cases in older subjects, he substituted astragalectomy for the cuneiform tarsectomy.

In the cases in children in which he did complete tarsectomy, he seldom divided either the tibial tendons or the ligaments at the inner side of the ankle, as those structures did not constitute the real obstacle, and moreover, they were readily stretched.

He thought that Mr. Jones's plan of always wrenching the foot before attacking the bones was excellent. He quite agreed with Mr. Jones that much of the apparent adduction of the foot was often due to a spiral twist of the bones of the leg below the knee, and that it was not due to rotation inwards of the limb at the hip-joint. If Mr. Jones's plan of repeatedly manipulating the limb so as to untwist the tibia be adopted, he agreed with the previous speakers that there was no need to condemn the child to the wearing of irons extending up to the pelvis. Nor did he (Mr. Stiles) believe in the elaborate and complicated club-foot boots or other forms of harness which apparently some orthopaedic specialists still considered it necessary to employ. A simple splint, such as that shown by Mr. Jones, was, however, very useful in the after-treatment in some cases. He (Mr. Stiles) generally used plaster-of-Paris, especially in hospital patients. He was of opinion that a radical operation, followed by placing the foot in plaster-of-Paris in the overcorrected position was, on the whole, the simplest way of treating bad cases of congenital talipes equino-varus, especially in the poorer classes.

He could not agree with Mr. Owen about the value of Phelps's operation. He thought the principle of the operation was wrong, and he had abandoned it long ago; the worst cases of relapse he had seen had been after the Phelps operation. Mr. Stiles said that the part of the deformity he had found most difficult to correct was not the equinus or the varus, but the adduction of the metatarsus and anterior tarsus. His experience had been that neither division of the invertors and plantar fascia nor the removal of bone sufficed to remove the pigeon-toe deformity which resulted from this adduction. During the last few years he had been in the habit of dealing with this very troublesome condition by resecting the abductor hallucis muscle through a special incision along the inner border of the tarsus. The muscle, by taking origin from the inner tubercle of the os calcis, and being inserted into the base of the first phalanx of the great toe, acted like a bowstring, and consequently adducted the anterior part of the foot at the mid-tarsal joint.

Speaking for himself, he might say that he intended to seize the first opportunity of seeing how Mr. Jones used the wrench, because he felt that he had much to learn from him regarding the value and the use of that instrument.

Mr. A. H. Tubby, M.S. (London) said: The points to which I shall allude are the reasons for incomplete cure and the causes of relapse and their remedy.

Speaking on broad grounds, we may say that failures in the successful treatment of club-foot are due to three causes:

1. A perverse way of attacking the problem—meaning by this either division of the tendo Achillis before the front part of the foot is fully rectified, or division of the tendo Achillis at the same time that the front part of the foot is operated upon. The result in either case is the same—a fixed point, the heel held firmly by the contracted tendo Achillis, is lost; and therefore there is no point of leverage left for bringing pressure to bear in unfolding the front part of the foot.
2. A want of appreciation of the obstacles to complete reduction.
3. Insufficient retention of the foot after it has been reduced and inadequate supervision during the years of growth.

Experience teaches us that the appearances in imperfectly treated, neglected, and relapsed congenital talipes equino-varus may be grouped under four headings:

1. Insufficient dorsiflexion of the ankle. This is undoubtedly the chief cause, and exerts its effect in nearly every case.

2. A general inward twisting of the structures of the foot in front of the ankle-joint. The foot is rigid and resistant, and feels as if set in plaster-of-Paris. Marked pigeon-toe and adduction of the smaller toes are characteristic of this type. The foot shows a general curve inward, and its inner edge forms a circle of much less diameter than the outer edge.

3. The inwardly twisted or ball-heel type. When the patient stands the heels form ball-like prominences internally, particularly when viewed from behind, due to twisting of the os calcis and with it the front part of the foot. Rotation has taken place at the calcaneo-astragaloid joint, so that the inner surface of the os calcis looks upward and inward, instead of directly inward, and the outer surface downward and outward.

4. The inveterate club-foot, which combines the characteristics of the three foregoing types; the imperfectly dorsiflexed ankle, the pigeon-toes and generally in curved foot, and the ball-like heel.

With the exception of a few cases of generally incurved foot, I am convinced that the dominating cause of persistent deformity is inability to dorsiflex the ankle sufficiently. Limitation of power in this direction causes the patient to inturn the foot so as to relax the tension on the parts about the inner side of the ankle. The patient is also unable to bring the heel to the ground in walking, and he walks on the toes. They, by reason of the varus portion of the deformity, already have an inclination to adduction, and he finds it easier with a raised heel to walk with the foot further inverted than straight to the front.

A spurious form of inversion of the foot must not be overlooked, in which the foot points inwardly because the bones of the limb are rotated, sometimes as much as 90 degrees. In such cases the relationship of the foot to the leg can be and is made perfect, but the foot is turned inward on account of the excessive rotation of the limb on its axis. The treatment of these cases by operation and otherwise has been described elsewhere.

Loss of power of flexion at the ankle is due to three causes: Contraction of the tendo Achillis and of the deeper muscles of the calf, shortening of the posterior ligament of the ankle-joint, and bony malformation. Before we commence treatment we often obtain a hint as to where the difficulty lies. If we examine infantile club-foot, and find that on the outer aspect of the astragalus there is a bony projection, the obstruction to perfect reposition lies in the astragalus, and such cases require operations on that bone sooner or later. Also, after a tenotomy, we may suspect the existence of bony locking if section of the tendo Achillis, followed by forcible movement, allows very little increase of the angle of dorsiflexion. Lastly, if on bending up a foot as far as possible, the tendo Achillis does not become taut, then we know that there is bony locking. This, as will be shown, is due to malformation of the astragalus.

Investigations have been made by me in a number of relapsed cases on the condition of the astragalus, and they have induced me to perform excision of that bone at a much earlier age than is usual. In Fig. 1 a foot of this type is outlined, and in Fig. 2 the appearances and position of the bones during the course of astragalectomy are depicted. The upper and outer non-articular portion of the astragalus is seen to form a more or less quadrilateral surface, A, interposed between the tibia, T, and cuboid, P, effectually preventing complete upward flexion of the foot. In fact, before the anterior ligaments are divided this abnormal surface appears to be directly continuous, and is, in fact, approximately in the same plane as the outer and anterior surface of the tibia. The appearances depicted in Fig. 2 show how complete the bony block is. Figs. 3, 4, 5, and 6 are taken from astragali removed from relapsed and imperfectly cured congenital talipes equino-varus; and here I may be permitted to thank my friend and colleague, Mr. Rock Carling, for his kindness in making the illustrations for this paper.

It will be noticed that in Fig. 6 the head and neck of the astralagus are inverted, but in all we see at B a prominent bony tubercle, the prefibular tubercle, which is usually felt in these cases, even in infants, on the upper and outer aspect of the foot. The anterior edge of the articular surface of the astragalus forms a prominent lip (R, Fig. 6), obviously parallel with and projecting against the anterior edge of the inferior articular surface of the tibia, and rendering it absolutely impossible to force the astragalus further upward; the impossibility being due not only to locking of the bony edges in contact, but to the fact that the bulk of the astragalus is greatly increased in front of this point; an increase which commences at the anterior and outer extremity, is exaggerated throughout the quadrilateral rough surface, A, and reaches its culminating point in a ridge, R, at the anterior part of the ankle-joint. It is interesting to observe that in Fig. 5 there is at E a bony facet developed for the articulation of the astragalus with the internal malleolus; and in Fig. 8 the outline of the astragalus, viewed from above the summit of the bony ridge, is well shown. These illustrations will, I venture to think, prove my contention that in the abnormal overgrowth of the prearticular portion of the astragalus, culminating in an anterior articular ridge, R, and in the twisting of the neck, we have the explanation of this persistent inability to dorsiflex and evert the foot.

The treatment is obvious—remove the obstruction. And the question arises, Shall a part or the whole of the astragalus be taken away? By way of parenthesis it may be remarked that we entirely fail to see how wrenching can overcome the difficulties. Some surgeons may contend that in doing so we are able to remould a cartilaginous or comminute a bony astragalus into an entirely new and appropriate shape, so that it will fit easily into the mortice formed by the tibia and fibula. Again, division of the inferior tibio-fibular ligaments is seldom of any use, because the interosseous membrane is left untouched, and therefore we must remove bone. Formerly I contented myself with taking away the upper half of the astragalus along a horizontal axis, but relapse occurred. In some cases there remained incurvation at the medio-tarsal joint, due to insufficient care in maintaining overcorrection of the part, and in other cases the remainder of the astragalus became deformed and formed a prominent obstruction, which reproduced the abnormal conditions. The lower half of the astragalus was retained in the belief that it would serve to maintain the foot straight after operation, but care in the after-treatment will do this just as well. So now complete astragalectomy is done; and when it is clear to me that the obstruction is of this bony nature, I feel no hesitation in doing astragalectomy, even about the age of 3 years, as the results are so uniformly good.

Astragalectomy has this merit: By means of it we can not only secure complete dorsiflexion, but also correct a considerable part of the inversion. We must, however, not trust to it alone. A good mode of procedure is to use every possible means, such as tenotomy, wrenching, and manipulation and remodelling, to reduce the inversion, and only when we are satisfied that the persistence of deformity is due to bony obstruction are we justified in removing the astragalus. We are also compelled in some inveterate adolescent cases to remove a wedge from the tarsus as well. Tarsectomy in any form is not often justifiable for mere inversion, but astragalectomy is frequently called for to overcome the dorsiflexion, and gives a useful, shapely and supple foot.

REPLY.

Mr. ROBERT JONES, in reply, emphasized the causes of relapse and their prevention, and made a plea for simple methods and appliances. Whatever plan the surgeon adopted he should be sure of overcorrection of deformity; nothing less would do. He disagreed with the principle underlying Phelps's operation, which involved a contractile scar on the inner aspect. He had seen many relapses after this operation performed by good surgeons. If relapse occurred with the large internal sac it would be irremediable. Lund's operation of excision of astragalus as advocated by Tubby was inadvisable in any but the severest deformity. An abnormally shaped bone could be less drastically treated. Mr. Jones felt it was better that each surgeon should effect his purpose by the methods most familiar to him. He should, however, not allow himself to fail in producing an artistic result.

TO ILLUSTRATE MR. TUBBY'S REMARKS.

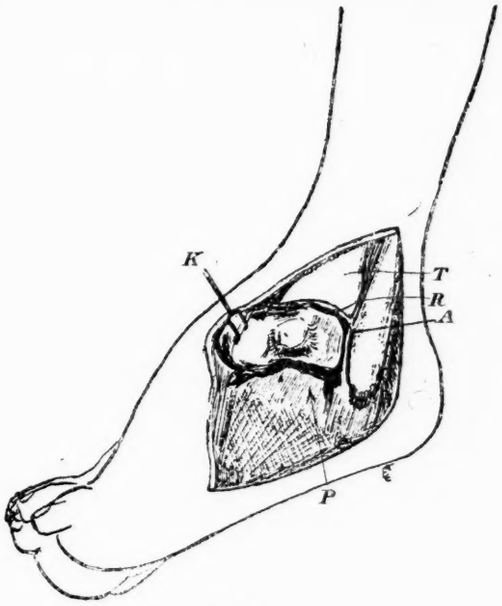
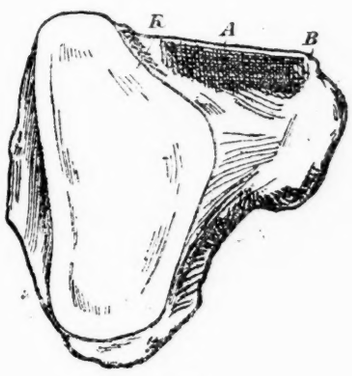


Fig. 2.

Fig. 1.

Fig. 7.



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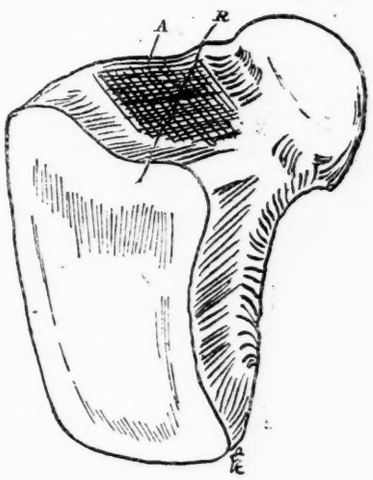

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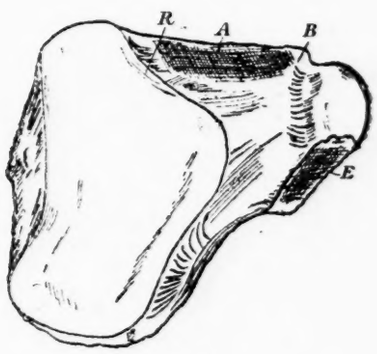

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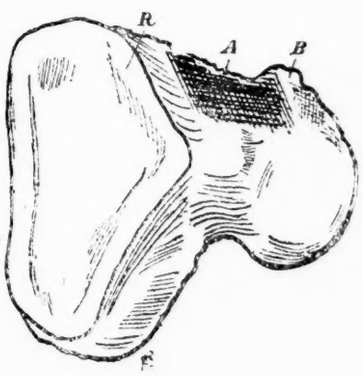


Fig. 6.

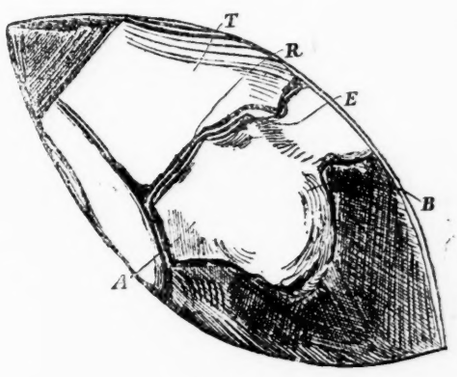


Fig. 8.

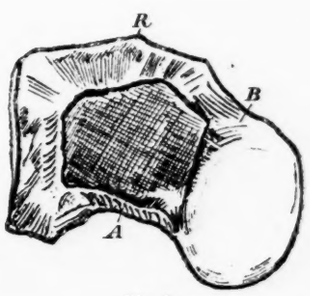


Fig. 9.

Fig. 1.—View of a left foot, with relapsed congenital talipes equino-varus, in a child of 12 years of age. At H is seen the prominence of the outer part of the neck and head of the astragalus. (For this and the following illustrations the writer is much indebted to his colleague, Mr. E. Rock Carling.)

Fig. 2.—Illustrating the position of the bones in Fig. 1, and the obstruction formed by the rough quadrilateral surface (A K) on the outer part of the neck and head of the astragalus, preventing normal dorsiflexion of the ankle. T, Tibia; P, cuboid.

Figs. 3, 4, 5, 6.—Illustrations of some astragali removed by the writer for relapsed congenital equino-varus. In all, A = rough surface on the outer aspect of the head and neck of the astragalus; R = bony ridge, which locks against the lower end of the tibia; B = bony tubercle (prefibular tubercle) at anterior end of abnormal external surface of head and neck of astragalus; E = new facet for articulation with internal malleolus.

Fig. 7.—View of right foot from a case of relapsed congenital equino-varus. H, Bony prominence on outer side of head and neck of astragalus.

Fig. 8.—View of the parts in the case in Fig. 7 during an operation for astragalectomy. T, Tibia; A B, abnormal surface on the astragalus; R, ridge on the ankle-articular surface of the astragalus; E, abnormal facet for the internal malleolus.

Fig. 9.—View of an astragalus from a severe case. A B, Quadrilateral surface; R, summit of bony ridge, causing locking of the ankle-joint.

Memoranda:

MEDICAL, SURGICAL, OBSTETRICAL.

SQUAMOUS-CELLED EPITHELIOMATA.

IMMUNITY NEARLY TWO YEARS AFTER OPERATION.

A. H., male, aged 61, had a small ulcer on the highest point of the helix, which ulcer had commenced as a sore place owing to an accidental cut by a barber while having his hair cut in May, 1907. It had remained sore and continued to grow until I saw him on December 6th, 1907. It was then about $\frac{5}{8}$ in. in every diameter, with a raw surface at the top; there was very little pain, and no enlarged glands could be felt in the neighbourhood. The growth with a wedge-shaped piece of the ear was removed under cocaine anaesthesia. The wound healed by first intention.

W. P., male, aged 62, was seen in December, 1907, with a "sore place" on the tip of the great toe, which he said started as a corn some nine months previously, and in spite of numberless corn cures and ointments had gradually got larger. The growth was $\frac{3}{4}$ in. in diameter, with a raw surface, and about $\frac{1}{2}$ in. in depth. The last joint of the toe was removed under cocaine anaesthesia. The femoral glands were not enlarged until after the operation, when owing to some suppuration one enlarged and inflamed, but quickly subsided.

W. B., male, aged 61, was being treated in January, 1908, for influenza, when I noticed a small sore about $\frac{1}{8}$ in. in diameter on the left upper eyelid. He then told me he had noticed a little sore place there for over a year which periodically scabbed over, but each time he picked off the scab. There were no enlarged glands in the vicinity to be felt.

The interest in these three cases lies, first, in that microscopically they were all proved to be squamous-celled epitheliomata; secondly, they were not considered by the patients to be of sufficient importance to lead them to consult a doctor, as all the three were being treated for something else when brought to my notice; and thirdly, the immunity from other symptoms for nearly two years.

Middlesbrough. THOMAS M. BODY, M.R.C.S.

THE CAUSE OF DYSMENORRHOEA.

A BENGALI lady, aged about 24 years, gave a history of dysmenorrhoea before her marriage, at times so painful as to require the use of morphine. Eventually she was prevailed upon by her relatives to submit to local treatment. The cervix was dilated under an anaesthetic, and kept dilated by bougies passed at intervals for some months afterward. This afforded her great relief. She was married about three years ago, and has since had two perfectly normal, and by no means painful, labours. Her last child was born a year ago; I attended her in that confinement. She has had no return of the dysmenorrhoea since her marriage.

Three weeks ago I was called to see her, and she informed me that she had a return of the old pain. She was in the third day of a so-called period, and was suffering acutely. As, however, there was a history of a scanty period the previous month and clots had been passed I insisted upon an examination, and found, tightly gripped in the cervical canal, a mass of clot and membrane, which was with the greatest difficulty removed by the finger. On attempting to pass the finger into the uterus to remove any remaining products of conception I found distinct spasm of the internal os, which was with difficulty overcome. This manœuvre relieved the pain somewhat, but spasms of pain continued to occur daily for four or five days with decreasing intensity until the discharge ceased. The discharge itself was quite normal, and only contained shreds of membrane and small clot. The pain curiously came on regularly at about 11 a.m., and, if not relieved by morphine, continued to increase until about 2 p.m. and then gradually died away, until by the evening it was merely discomfort. As the continuance of the pain suggested that the uterus still contained clots, I examined several times both before, during, and after the spasms of pain; and whilst the cervical canal remained patent, one could distinctly feel a constricting band at the upper end of it, almost impossible to dilate with the finger so long as the pain was present. Before and after the spasms this was not noticeable. The patient herself remarked that a few hours before the pain came on the discharge increased in quantity, but was diminished while the pain persisted and again increased as the pain passed off.

I make no comment upon the above fact, but it seems to me to supply the missing link demanded by your correspondent in your issue of May 15th.

Assam. F. C. MCCOMBIE, M.B.Lond.

TREATMENT OF TRAUMATIC TETANUS BY ANTITOXIN SERUM.

S. M., aged 28, sustained a lacerated wound of the fleshy portion of the second finger of the left hand, caused by a hook of a window-fastener on the evening of March 24th, 1909. The wound was dressed antiseptically on the same evening and began to heal. On the morning of April 7th, fourteen days after the injury, he complained of stiffness of the left jaw, cramp on the left front of his chest over the cardiac region; later in the day the abdominal muscles became tense on pressure, and there was spasmodic contraction of the dorsi-lumbar muscles. The case was seen in consultation by Dr. Pearce. The temperature was 97°, and the pulse 54. The patient was removed to hospital, and at 8 p.m. 20 c.cm. of antitoxin tetanic serum was given by the mouth.

During April 8th 100 c.cm. of the serum was injected subcutaneously, and a mixture containing chloral hydrate 15 grains, pot. bromidi 15 grains, was given every fourth hour. He suffered from slight spasms of the abdominal and dorsi-spinal muscles, from recurring cramps in the muscles of the calves, and inability to open the mouth beyond a very small extent; slight risus sardonicus was present, and there was a peering appearance of the eyes, but no photophobia. The bowels were constipated, and he was very restless and unable to sleep for long.

On April 10th he had severe spasms at 1 p.m., when he bit the right side of his tongue, making a jagged wound; 50 c.cm. of serum were again administered subcutaneously, and 50 c.cm. more within the twenty-four hours. No subsequent severe spasms occurred. He continued in much the same condition with slight recurring cramps in the calves of the legs and clonic contraction of the hamstring tendons. Opisthotonos was slightly marked. He slept in snatches, and hypodermic injections of morphine $\frac{1}{3}$ grain were given every second hour without beneficial result. Sodium bromide was substituted for potassium bromide as being less depressing. Subsequently the patient had more prolonged sleep. The wound of the finger had continued to heal except at a small pin-head point from which projected a small hair. On inquiry it was found that the patient, while the wound was healing, used the injured hand grooming a horse, though cautioned not to do so. The hair presumably conveyed germs to the wound, as I am informed the patient had even removed the dressings he felt the wound to be healing so well.

On April 14th the temperature was 100·3°, and 100 c.cm. of antitoxin serum were given. On April 17th the temperature was 101.1°, and 20 c.cm. of antitoxin serum were given. On April 18th he was much weaker and restless. The pulse was 80, small, and wanting in tone; the temperature was 101°. This condition lasted till the evening of April 19th. He had been given sulphate of magnesia to act on the bowels, which continued constipated.

Subsequently he improved steadily, was able to go into the garden on April 30th, and was discharged from hospital on May 12th.

H. V. MCMAHON-DILLON, L.R.C.S.I., L.R.C.P.I., L.M.

A CORRESPONDENT of the New York *Medical Record* calls attention to what he calls the "passing of sectarianism" in America. He says: "A homoeopathic college in Denver and one in Baltimore have in effect taken themselves out of the homoeopathic ranks. An old homoeopathic college in St. Louis has closed its doors. The board of regents of the University of Minnesota has practically abolished homoeopathy in that institution. It is said that some of the Iowa authorities are growing impatient over the burdensome homoeopathic department in that university. There remain some three or four strong and well-equipped homoeopathic colleges, which are largely reduced to a modern basis but which remain nominally sectarian."

Reviews.

DIETETICS.

The excellent work on *Diet in Health and Disease*,[1] by Drs. Friedenwald and Ruhräh, has now reached a third edition, and has been revised in order, as the authors say, to make it more valuable to the practising profession. The articles on milk and alcohol have been rewritten, and additions have been made to those on tuberculosis, salt-free diet, rectal feeding, the caloric needs of infants and others, while several useful tables showing the caloric value of foods have been added, as well as one showing the composition of diabetic foods, and a short account of the simpler methods for detecting food adulterations and preservatives. These additions have been made without any appreciable increase in the size of the book, and will doubtless be appreciated by its readers. The chief value of this work lies in its tabulated information. There are, for example, a number of dietaries of public institutions, prisons, lunatic asylums and hospitals, and the dietaries of the army and navy, besides special diets for various diseases which are all given in great detail. Here can be found accounts of all the various test meals used in stomach diagnosis, with full directions for their administration. On the other hand, there is very little in the way of general discussion of principles. Chittenden's experiments, for example, receive quite inadequate discussion in their bearing upon the general question of diet, and are not even alluded to under the subject of the training of athletes. The references and descriptions of diets are almost exclusively drawn from German authorities; and as the food eaten in Germany is so different from that used in this country, they are not as convenient as they might be to the British practitioner. It is somewhat remarkable to find Haig's diet not even mentioned, although in certain diseases it is undoubtedly useful. Milk soured by the growth of the lactic acid bacillus is mentioned, but there is no reference to its dietetic value in the treatment of chronic constipation. The very interesting subject of idiosyncrasies in diet is dealt with in quite too brief a manner. It may be worth noting that the authors say that sea-water fish should be eliminated from salt free diet, as such fish contain a large amount of sodium chloride. The authors possess the apt American way of putting things. For example, in their article on milk, they say: "It costs less to buy a baby good milk for a year than to bury it."

The little book on *The Dietetic Treatment of Diabetes*, by Major B. D. Basu,[2] a retired officer of the Indian Medical Service, is stated in the preface to have been written "as much for the general public as for the ordinary medical practitioner." It is mainly composed of judiciously selected extracts from the writings of authorities on the subject, but to some extent it reflects the author's personal experience among the natives of India, where diabetes is common, and where, also, owing to the climate and local habits, it probably runs a somewhat different course to that generally met with in Europe. We gather from the author that diabetes in India, as a rule, is a mild disease which lasts many years. This he attributes to the vegetarian diet of the people and their abstinence from alcohol. How far the former statement may be true we are not in a position to say, but the disease in Europe in elderly people usually runs a prolonged course, although our patients eat a certain amount of meat and take a little alcohol, and without further evidence we are not prepared to admit that "diabetic patients who are vegetarians live longer than those who are meat eaters." We gather that the author is in favour of treating diabetes in natives of India by a strictly vegetarian diet; he certainly condemns the practice of European doctors who in treating such cases prescribe meat; but it would have been interesting if he had given us diet tables showing the kind of food upon which he has been able to manage moderately severe cases of diabetes successfully without the use of animal food or milk. He objects to milk "at all periods of the disease," as in his experience it has always increased the glycosuria; but, if this observation is founded upon a sufficient basis, it does not agree with European results when milk is given in small quantities. He speaks of bread made from "fish roe dried and ground," but tells us nothing about its palatability or the kind of fish from which the roe for this purpose is obtained. It is misleading to speak of saccharine as a "poison," for in small quantities in which it is used as a sweetening agent it appears to be harmless. The author's opinion upon the use of alcohol is apparently adverse, but the extracts given are in its favour. He quotes a far from convincing article by Dr. Ridwill, of Melbourne, on the use of rye bread in diabetes, which was followed by the "happiest results"; it is said to be satisfying, but not to cause any increase of sugar; this is explained by the starch not being digestible, but the digestion experiments upon which the explanation is based were made not with saliva, as one would expect, but with pepsin, hydrochloric acid, and pancreatic extract! The book is disfigured by numerous misprints which occur on almost every page, while the style is marked by that capricious use of the article which is so characteristic of English as written by natives of India.

[1] *Diet in Health and Disease.* By Julius Friedenwald, M.D., and John Ruhräh, M.D. Third Edition. Philadelphia and London: W. B. Saunders Company. (1909.)

[2] *The Dietetic Treatment of Diabetes.* By B. D. Basu. Allahabad: The Panini Office. 1909. (Cr. 8vo, pp. 40. Rs. 1.8.)

GENERALITIES.

In a pamphlet on the essential nature of disease[3] Professor Ribbert discusses in philosophical fashion the underlying principles of cellular pathology, the significance of the pathological tissue changes associated with the origin of disease, and the pathological aspects of heredity. He defines disease as "the sum of the lowered vital processes which are dependent on changes in the structure of the body due to an absence of accommodation." Virchow's three-fold division of cell stimuli into the functional, the nutritive, and the formative is discussed at great length; it cannot be regarded, in Professor Ribbert's opinion, as a sound basis for the explanation of morbid processes. "Physiological stimuli, as such, obviously have no pathogenic significance. But if they become too intense they damage the organs concerned. So it is with pathogenic stimuli. Whilst in lower degrees of intensity they only give rise to functional activity, when they are stronger they produce damage. But when operative with these injurious degrees of intensity they can never exercise either a nutritive or a formative influence. There is no such thing as a nutritive or formative stimulus produced by pathogenic influences." The author's theories about cell stimuli must be taken in conjunction with his doctrine of tissue growth and tumour formation. He regards all growth as an exercise of a "capacity for growth" which is always immanent within the cell, and is brought into activity by a release of the normal "tissue tension." This change may be due to a variety of causes, such as the formation of gaps in a tissue (in regeneration and in compensatory hypertrophy), hyperaemia (in inflammation), other mechanical causes, the escape of cells from their normal situation in a tissue owing to chemiotactic influences, and the displacement of cells from the normal restraints of their environment which occurs in tumour formation. By an elaboration of these considerations the author arrives at his well-known and oft-reiterated views as to the origin of cancer. In the concluding chapter of the book he protests against the improper use of teleological conceptions. For example, the progressive changes which lead to healing must not be regarded as "purposive." They are not produced by an agent acting with a view to an end, but arise as the necessary consequence of the innate properties of the tissue. The changes commonly regarded as purposive would more properly be described as the manifestations of a capacity for accommodation to the conditions of life. As the author admits, some of his theories are still the subject of controversy, and we doubt whether all objections to them have been removed in the present volume, which contains the latest exposition of his favourite doctrines. We refer in particular to his attempted explanation of the origin of cancer by a theory of disturbed "tissue-tension." Whilst everybody admits that in cancer formation there must, *ipso facto*, be a disturbance of normal intercellular relationships, many pathologists

[3] *Das Wesen der Krankheit.* Von Dr. Hugo Ribbert. Bonn: Friedrich Cohen. 1909. (Roy. 8vo, pp. 173. M. 4.)

fail to regard the conception of disturbed "tissue-tension" as anything more than a restatement of obvious fact in the language of fanciful metaphor; they consider that the use of the word "tension" should be restricted to the precise meaning which it bears in the science of physics. As a teacher of morbid histology and anatomy, Professor Ribbert's gifts of accurate observation and lucid exposition have secured for him an undisputed position in the very first rank, but as an authority on the more speculative and theoretical aspects of pathology, his claims to pre-eminence are less well established.

Dr. RICHMOND's aim in writing his *Essay upon Disease, its Cause and Prevention*,[4] has been "to point out the very large number of diseases which are either spread by food or directly due to impurities found in articles of diet or articles in common use." The essay has, admittedly, been written hurriedly—to which, indeed, it bears inherent witness. After a somewhat elementary exposition of the author's views of "Life, Health, and Disease,"—in the course of which he deduces, first, the "law of constancy of function," according to which cells which have identical functions must have identical chemical composition; and, secondly, the "law of permanency of function," by which every cell in the organism will continue to perform its function normally, unless some influence or poison is brought to bear on it, which interferes with the usual decomposition of its cell substance—he passes to the influence of diet and environment on disease. This part consists mainly of simple and often questionable statements, unsupported by evidence or authority. We give one or two of these precisely as they are printed, in all their amazing baldness and absence of contextual reference.

> Conjunctivitis, suffusion of eyeballs, tinea tarsi, caused by diet, and in all probability errors of refraction, and increase of ocular tension and buphthalmos.
>
> Ringing in the ears, deafness, catarrh of middle ear, and pain in the ears, deficiency of wax, excess of wax, can all be caused by such articles of diet as cheese and bread, etc.

Similarly, neuralgia is caused by milk; hysteria in women by toxins in tea, butter, etc.; asthma is "probably due to substance in some article of food or drink"; and, alas! toothache to tobacco. Out of evil, however, good may come, for the author states that it is possible to control the colour of the hair by diet, this being shown "by the Bible stating that Jacob was able to make the sheep bring forth lambs with spots or without, by giving them certain kinds of food to eat." This is the sole authority quoted by the author in this book, and unfortunately, we fear, incorrectly, for we have never heard it suggested that the goats and sheep *ate* the rods of green poplar and hazel and chesnut tree artfully set before the flocks by Jacob to produce what is known nowadays as a maternal impression.

SELF-EDUCATION.

"MAN is the only animal who does not know how to live." This is the opening sentence of Dr. PAUL DUBOIS's introduction to his book, a translation of which by Mr. H. H. BOYD has recently been issued under the title, *Self-Control and How to Secure it*.[5] The title may be regarded as the text of a comprehensive treatise on the means by which it is possible to mitigate functional faults of inheritance, to correct those which we all acquire, and to educate ourselves generally on such lines as will make it possible for us to make the best of our opportunities and get the most out of our lives. The desire for happiness is, so says the author, the guiding motive of human life. He admits that real and lasting happiness is not to be found in the realization of desires, however legitimate they may be, and, though he evades any definition of what true happiness is, he contends that those come nearest to its attainment who observe best the virtues which, according to the doctrines of religion, assure for us felicity in a future life. These virtues are only to be acquired by the education of self and by the development of conscience, which is defined as "a collection of moral conceptions which, at a given moment, exist in the understanding of man and serve him as a guide for the conduct of his life."

In elucidation of his argument the author considers such abstract subjects as meditation, tolerance, humility, patience, courage, sincerity, and kindness—to name a few only—and under each heading we find an interesting and philosophical dissertation which arrests attention, although the argument may not always be so convincing as to command the unqualified assent of the reader. Dr. Dubois strongly urges the need for self-examination as an essential factor in the educative process, and rightly contends that no discouragement need ensue from the discovery of faults and failings if we but use past experience to improve the future, and accustom ourselves to live in an atmosphere charged with a moral perfection which constantly stimulates our endeavours to betterment. Meditation is a habit essential to ethical development, and action is the psychological expression of feelings which, in the case of the commoner virtues, become automatic. To the majority of people who have acquired an average moral control, vulgar crimes like theft, forgery, or murder are inconceivable; the firmly-established conviction that such things are wrong makes their commission impossible. By a wider and more careful cultivation of the moral sense the author argues that the practice of such other virtues as tolerance, patience, chastity, and kindness will ultimately create a similar psychological automatism, and make their daily practice a mere matter of course. The following passage brings home forcibly one aspect of defective self-education with which most men are familiar:

> We lack indulgence and patience towards our fellow-beings when, without being really ill, they exhibit those variations of humour to which we are more or less subject. Under the influence of fatigue that apparently is not justified by the work done, when in states of organic disturbance caused by certain physiological and psychological phenomena peculiar to our being, we feel influenced in our mental life; we become sullen and discouraged, without serious reasons; we appear rebellious, wicked, and we are sorry, but our nerves govern us, and we are unable to expel the enemy that is within us. We should more easily escape if those who surround us had determinist indulgence in their hearts; if they could recognize our weaknesses as their own. They forget that they are not always what they would like to be, and they nag us harshly. Impressionable people often suffer real martyrdom in their families. Subject to constant variations in their state of mind they are misunderstood, and the reproaches they receive take away their last vestige of self-control. Doubtless a word may do them good, on occasions even a reproach, provided it be friendly. The person who is impatient and fretful suffers; he does not feel well without being able to say what is the matter. We should regard him as a patient who needs repose or encouragement, and not as a culprit who is willingly sullen.

This is a graphic and very human picture, and its widespread application indicates the need for the cultivation of sympathy, which, after all, is the practical expression of personal experience, and of a philosophic adaptation of self to the circumstances and exigencies of life. Dr. Dubois's book is full of thoughtful and suggestive references, and its perusal cannot fail to promote in the reader a true desire to improve the ego, which it is his business to control and his duty to educate. Mr. Hutcheson Boyd is to be congratulated upon the fluency of his translation; but the printers have put the pages so carelessly together that, in the middle of the volume, one or two of the chapters have become mixed up in hopeless confusion.

MYXOEDEMA AND CRETINISM.

FOURTEEN years have elapsed since the appearance of the first edition of Professor EWALD's book on diseases of the thyroid, myxoedema, and cretinism in the Nothnagel series of handbooks. The second edition, which has been brought up to date and almost entirely rewritten,[6] will confirm the favourable opinion expressed in our review of the first edition (1896, vol. ii, p. 655). In the sections on anatomy and physiology the chief alterations are due to the recognition of the importance of the parathyroids. Their anatomy is carefully described, the vitiation of the conclusions formerly drawn from experiments in which

[4] *An Essay upon Disease, its Cause and Prevention.* By G. E. Richmond, M.D., B.Sc., B.A.Lond., D.P.H.Camb. London: H. K. Lewis. (Cr. 8vo, pp. 96. 2s.

[5] *Self-control and How to Secure it.* [L'Education de Soi-Même.] By Dr. Pau lDubois, Professor of Neuro-Pathology in the Universit y of Berne. Authorized translation by Harry Hutcheson Boyd. New York and London: Funk and Wagnall's Company. 1909. (Demy 8vo, pp. 337. Price 6s.)

[6] *Die Erkrankungen der Schilddrüse, Myxoedem und Kretinismus.* By Dr. C. A. Ewald. Second edition. Vienna and Leipzig: Alfred Hölder. 1909. (Sup. roy. 8vo, pp. 297; 26 illustrations and map. M. 8.80.)

they were ignored is continually kept in mind, and mention is made of the recent work tending to prove that their removal is responsible for the onset of tetany and other post-operative catastrophes. There is also some timely criticism of the various hypotheses advanced in regard to the inter-relationship of the thyroid and the pituitary. In dealing with cretinism the varieties (sporadic and endemic cretinism) are distinguished from juvenile myxoedema and from Mongolism and achondroplasia—conditions often confused in Germany. Careful and complete descriptions of the clinical and anatomical appearances in cretinism and myxoedema are given, and compensate for the very poor illustrations. In place of a long review of the numerous theories to account for the endemic occurrence of goitre and cretinism there is a most interesting exposition of the "drinking water" theory, illustrated by many striking examples of epidemics and of the disappearance of these diseases from an endemic area when a proper water supply has been obtained. The importance of the subject is shown by statistics, perhaps the most striking fact being the statement that the Swiss army loses about one-sixth of its numbers from cretinism alone. In regard to the treatment of goitre the author still pins his faith to iodine in its various forms. He is very sceptical about the alleged dangers of iodide in goitre, but is too enthusiastic about the virtues of iodine and too pessimistic about the dangers of surgery. There is an excellent chapter which sums up our knowledge of thyroid feeding, not only in goitre, cretinism, and myxoedema, but also in other conditions in which it has been recommended, such as psoriasis, obesity, and acromegaly. The phenomena and causation of thyroidism are discussed at length, a long list being given of all the ill effects which have been ascribed to thyroid feeding. Here, as throughout the book, a statement of the evidence is accompanied by carefully weighed criticism which, coming from such an eminent authority as Professor Ewald, is well worth serious consideration. The bibliography contains over 1,400 references, but they are not well selected. Many of them are English, but appear to have been culled from the index columns of periodicals, and refer in many cases to single patients shown at societies, whilst on the other hand such a valuable monograph as that of Berry is not mentioned. As a work of reference the book is ruined by two faults: there is no index and the reference to an author in the text gives only his name, although in the bibliography we may find half a dozen references under it.

ACCIDENTS TO WORKMEN.

THE volume on *Accidental Injuries to Workmen with reference to the Workmen's Compensation Act, 1906*, by H. NORMAN BARNETT, F.R.C.S.,[7] is intended to supply some of the needs of those who are called upon to deal with such matters, both from a medical and a legal point of view. The author was assisted in his work by Dr. Cecil Shaw of Belfast, who devotes some twenty-seven pages to injuries to the organs of special sense, and by Mr. Thomas J. Campbell, M.A., LL.B., B.L., who deals in an interesting manner with the law in relation to accidents and industrial diseases. The appendices contain the text of the Workmen's Compensation Act, 1906, a list of industrial diseases, and the report of the Departmental Committee. The book contains 384 pages, and of these 196 are devoted to injuries and diseases. The chapter giving directions for making a report on an injured workman will be of value to those who have little experience of this kind of work, and the form of certificate in use by a certain assurance corporation is filled in for the guidance of the tiro. Dr. Barnett has collected most of the facts as to etiology and diagnosis which we find in the ordinary textbooks on medicine and surgery, and presents them in an accessible and readable form. The modifying effect of pre-existing disease is considered at some length, and the importance of a medical report as to the physical condition of candidates for employment is noted. In this connexion we believe that in some districts workmen have resisted such an examination, and neither insurance companies nor employers have insisted, nor does the Act itself require it, so it is probable that the test of good health, which is often applied to a claimant, namely, capacity to earn average wages previous to the alleged accident, will hold good in individual cases, unless there is strong evidence proving the existence of disease antecedent to the injury. The information presented in the various sections will be of service, at any rate in the less contentious cases, and the medical man who is occasionally required to examine and report with a view to giving evidence in Court, will find the book helpful. Some of the sections are rather too brief, and might be made more comprehensive in another edition. Questions of diagnosis are dealt with shortly, but quite clearly, and the points are set out in a manner which will serve to refresh the memory of the medical examiner. Under the heading traumatic neuroses, the author warns the medical examiner against falling into the error of regarding the injured workman as an impostor or a pure neurotic, while at the same time due regard is to be given to the possibility of malingering. The work is in clear type, easily read, and will no doubt serve a useful purpose.

[7] *Accidental Injuries to Workmen.* By H. Norman Barnett, F.R.C.S. London: Rebman, Limited. 1909. (Demy 8vo, pp. 384. 7s. 6d.)

THE SIGNS OF DEATH.

DR. ICARD has done useful work in calling the attention of the profession and the public, in his monograph upon the signs of death,[8] to the various methods by which the fact of death can be proved. He discusses with some fullness the various signs of death, dealing with especial care with the "fluorescine test." He points out, however, that all the signs used up to the present require skilled help for their demonstration or reading, and that it is exactly in those cases in which it is impossible to obtain the assistance of a doctor that there is a need for some test which will be both simple and easy of application and at the same time infallible. This, he considers, he has found in the reaction which he names the "*réaction sulfhydrique.*" According to Dr. Icard, any signs of putrefaction which may occur in the last stages of life are due to the action of aërobic bacteria, the action of the anaërobic bacteria never becoming evident until life is extinct. One of the first manifestations of the activity of these anaërobes is, he states, the evolution of sulphuretted hydrogen gas; and as the bronchial mucous membrane is one of the first parts of the body to show putrefactive changes, and, further, as in temperate or hot climates putrefaction of the bronchial mucous membrane commences within a very few hours of death, he considers that the demonstration of the presence of sulphuretted hydrogen in the gases coming off from the nostrils or mouth is proof positive of death having already occurred. The author tests for sulphuretted hydrogen by placing either a silver coin or a piece of paper moistened with neutral acetate of lead over the nostrils of the body. If life is still present, no sulphuretted hydrogen will come off and no change will take place in the coin or paper; but if life is extinct, then, in the course of a few hours, the coin or paper will become blackened. This test, he avers, never fails, and it is evident that it is not only simple in its method of application but definite in its results, and can be used and understood by the most ignorant. The author enters fully into the possible fallacies and objections to the test, and certainly shows that errors are at least not likely to occur with it, except in cases in which the surrounding temperature is very low. He contends that the possibility of premature burial is a real and not an imaginary danger, especially in those cases in which the supposed corpse has not been seen by a medical man. Such accidents are, he admits, rare, but he states that it must be allowed that they have occurred in the past, and that, as they may take place in the future, he thinks it of the greatest value to have some test which will serve to render so lamentable a catastrophe more infrequent still.

[8] *Le signe de la mort réelle en l'absence du médecin.* Par le Docteur Icard. Paris: A. Maloine. 1907. (Cr. 8vo, pp. 292, avec figures. Fr. 4.)

A SYSTEMATIC attempt to exterminate rats is to be made in all parts of the United States under the official sanction of the Department of Agriculture.

THE late Dr. Thomas Crawford Hayes, Emeritus Professor of Medicine at King's College, Consulting Physician to King's College Hospital, left estate of the gross value of £50,739 8s. 5d., of which the net personalty has been sworn at £48,765 7s. 3d. He left £100 to the King's College Hospital building-fund.

THE SIXTEENTH INTERNATIONAL CONGRESS OF MEDICINE.

BUDAPEST, AUGUST 29TH TO SEPTEMBER 4TH.

(*Continued from page 992.*)

SECTION OF MEDICINE.

THE Section for Medicine always attracts a good proportion of the members of a congress, and for this reason many authors prefer to read their papers before this section rather than before one of the smaller sections to which the subject might be referred. The number of *rapports* (or opening papers) was a large one, and included serum diagnosis, immunity, tuberculin treatment, appendicitis, arterio-sclerosis, and many others.

Clinical Value of Serum Diagnosis.

KRAUS (Berlin) read an interesting paper on the clinical value of serum diagnosis. He urged the clinician to have recourse more frequently than he did at present to laboratory methods of diagnosis. He gave his views with regard to the significance of the variations of the antiferments, to the complement deflection reaction for syphilis (Wassermann's), and to the specific diagnosis of ecchinococcal affections, tuberculosis, etc.

INMAN (London) then dealt with immunity in its relation to practical medicine. He put forward the views which have been taught by Sir A. E. Wright, and showed the same opsonic curves, which had been shown on numerous occasions in this country and on the Continent, including the occasion of the meeting of the *Naturforscher und Aerzte* in Cologne last year. This doctrine does not find much favour in Germany, and the lecture left the majority of those present unmoved. A discussion followed in which several clinicians differed from Kraus and Inman on their respective statements. CITRON suggested that it should be required of all wet nurses to have their blood subjected to Wassermann's test.

Tuberculin Treatment of Tuberculosis.

PEL (Amsterdam) was more successful in awakening active interest by crystallizing his communication into a concrete subject rather than dealing with general principles. He limited himself to the question of the tuberculin treatment of tuberculosis. He said that it was easy to understand that tuberculin was more effective in early cases; but the fact that early cases did well without any special form of treatment made it questionable whether the tuberculin actually contributed to the recovery. The matter could only be tested in cases which had remained stationary in spite of the most favourable conditions. There seemed to be considerable difference of opinion as to the indications for tuberculin, the dose, the selection of cases, and as to the local and general reaction. Pel did not approve of basing the indications on Wright's opsonic index, which he considered to take up too much time and to be too unreliable for practical application. The experience of the past few years in the sanatoriums had shown that selection of cases and indications made on the basis of clinical manifestations yielded very good results. It was undeniable that tuberculin treatment had its drawbacks. Not every practitioner was capable of applying it. An exact knowledge of the minutiae of the technique was required to render it tolerably safe, and even with this knowledge the practitioner must watch his cases carefully all through. The individual variations of susceptibility to tuberculin must be taken into account, and, inasmuch as tuberculin might easily do harm, the dose to begin with must be small, and it was necessary to avoid all local and general reactions. Again, care must be exercised in raising the dose, since anaphylactic symptoms frequently arose. Pel had seen haemoptysis occur as a result of this anaphylactic action. He said tuberculin was a dangerous friend, especially in the hands of those who had not completely mastered the method of application. If this treatment were adopted widely in general practice, he was convinced that it would do more harm than good. But he believed that further careful study both at the bedside and in the laboratory would render it the best weapon of defence against the most destructive disease to which the human race is exposed. MARAGLIANO (Genoa) spoke in favour of his serum. In the discussion CITRON (Berlin) also declared himself opposed to controlling the tuberculin by means of the opsonic index. The object of tuberculin treatment was to induce tolerance to large doses of the substance, but he was of opinion that the old method of attaining this end was dangerous. He then criticized the tuberculins and came to the conclusion that no perfect tuberculin had yet been introduced. He believed that the best results were to be obtained by using tuberculin together with serum (for example, Kraus's tuberculin). POTTENGER (Monrovia) was of opinion that every patient was treating himself automatically with tuberculin. The question to be decided was whether it was better to leave the patient alone to treat himself or whether some advantage could be gained by applying tuberculin in measured doses. Great care was required, but he was convinced that the latter method was advisable. The dose must be increased constantly, and he was of opinion that under certain conditions fever need not be regarded as a contra-indication.

Overcrowding and Tuberculosis.

BERNHEIM and DIEUPART (Paris) dealt with the question of overcrowding, imperfect hygiene, and certain trades in the production of tuberculosis. The measures which they advised should be commenced in the schools, and should have ramifications throughout every building and trade which might be responsible for a spread of infection.

Arterio-sclerosis.

HUCHARD (Paris) opened a discussion on arterio-sclerosis. He found that, in view of the large number of different opinions, it was impossible to set up an exact definition on the basis of morbid anatomy. Experimentation had been able to imitate the lesions but not the disease, and therefore no acceptable definition could be based thereon. From the point of view of the evolution, a sharp line of division must be drawn between atheroma and arterio-sclerosis. Atheroma was a disease of old age (between 60 and 80 years of age), while arterio-sclerosis occurred between 30 and 60. He was inclined to distinguish between valvular cardiopathies of an endocardiac origin and those of an endarteritic origin. The six principal causes of arterial cardiopathies were: Gout and uric acid diathesis, lead poisoning, syphilis, overfeeding, tobacco, and, lastly, mental overwork. The symptoms of cardio-sclerosis were toxic, cardio-arterial, and renal. Cardio-sclerosis might commence clinically by disturbances of the urine, of the heart, or of another organ. He concluded that arterio-sclerosis without a lesion of the arteries or of the kidneys did not exist. This definition enabled the observer to eliminate a large number of maladies, which were usually, though erroneously, included in the term "arterio-sclerosis."

MÜLLER (Budapest) spoke of abdominal arterio-sclerosis. He considered this affection as common, but that it was frequently not recognized as such. It was usually confused with neurosis of the gastro-intestinal canal. The diagnosis became easier when the attacks of pain were associated with cardiac and vascular changes. It attacked men more frequently than women, and was beneficially influenced by vaso-dilators.

V. KÉTLY (Budapest) regarded the process as a degenerative one. As a rule, the process began in the intima. Aortitis produced by syphilis must be distinguished from true angio-sclerosis. Those factors which produced long-sustained or often repeated increase in the blood pressure accelerate the development of arterio-scleresis. Even frequent exhausting mental work might lead to the disease. The toxic effects of tobacco, alcohol, lead, disturbances of metabolism, renal diseases, etc., played an important etiological part. Some authors believed that heredity also played a part. A sedative mode of life favoured a degenerative process of the vessels, especially if this were associated with overfeeding.

Polycythaemia.

SENATOR (Berlin) gave an excellent lecture on polycythaemia. He first briefly touched on what he termed "physiological polycythaemia," and then passed on to the consideration of the pathological occurrence of the condition. It was met with when the blood underwent pathological inspissation, in various intoxications and infections, especially when associated with haemolysis,

and also in various diseases showing certain changes in the blood. In all the cases save when there was a relative inspissation of the blood, the decrease of the oxygen tension of the blood must be regarded as the cause. Senator distinguished between polycythaemia and polycythaemia megalosplenica, and polycythaemia hypertonica. The last named was very rare. It was characterized by a very high blood pressure and no swelling of the spleen. In both of the two named varieties the blood showed an increased viscosity and specific gravity. In the treatment of polycythaemia a vegetable diet and blood-letting often did good.

Constitution and Diathesis.

There was a discussion on constitution and diathesis, in which the Section of Pathology and that of Medicine took part. The first paper read was one by His (Berlin), in the course of which he recorded his investigations on the real nature of what was called diathesis. He attempted to discover a definite symptom-complex occurring in several members of one family, or in several individuals of one type. The research required to be diligently continued for a long time, before final results could be given. For a clear understanding of many pathological conditions it was necessary to obtain a definite meaning of the word "diathesis." Martius (Rostock) dealt with the problem, especially with reference to heredity. He wished to define the word "constitution" in such a manner as to limit its application to general pathological conditions, and to exclude the changes underlying special pathological affections. In every disease constitution played a distinct part. He was of opinion that to speak of a non-constitutional or a constitutional disease was confusing the question. For example, phthisis was not constitutional, but a case of phthisis was influenced by the constitution of the patient. The constitution was frequently acquired, as in tabes, syphilis, etc. Biological investigations had lent a scientific basis to the doctrine of heredity. The author mentioned observations of this nature, and showed how a disease was often produced by an occasional determining cause, on a soil especially suitable for the further development of the disease process. He attempted to give a clear explanation of the changes which were inherited, in place of the old-fashioned hazy ideas. Lubarsch (Düsseldorf) started by stating that intrauterine infection and germinal carrying-over of disease were quite different from inheritance. He propounded the theory that there could be no such thing as an inherited syphilis; syphilis which was passed on to the offspring was a congenital infection. All inherited processes were determined when the seed joined the ovum. True heredity would be exemplified by a character or protection being passed on from the father to the embryo through the semen. The question of Weissmann's germinal theory and of Lamarckism was discussed, and Lubarsch stated that although a direct damaging of the germinal cells could produce changes in the being to be developed, neither this nor what Forel described as *blastophoric* influences could be identified with heredity. True heredity was, in his opinion, due to the intermixture of characters of the two parents, and was subjected to the play of chance. Finally he spoke of atavism, and illustrated his remarks by referring to the occurrence of haemophilia.

(To be continued.)

On Saturday, September 25th, the Lord Mayor of London laid the foundation stone of the new St. Mary's Hospital, Plaistow. The Mayor of West Ham having welcomed the Lord Mayor, the Bishop of Barking (chairman of the committee of management) said the hospital was doing excellent work, and King Edward's Hospital Fund had voted them £1,000 annually, to commence from the following January; the sum of £13,716 had been raised towards the £16,000 which was needed, and of this Viscountess Parker had collected a large sum. The Lord Mayor said the present hospital had become totally inadequate. The institution had developed out of a day nursery founded by the Rev. Given-Wilson in 1888, and, a small ward having been set apart for sick children, it finally became a hospital, from which at the present time, owing to lack of accommodation, cases were continually being turned away. The new building would contain 62 beds, and would be fitted with a large out-patient department and every modern appliance. The committee wished to open the new building free of debt, and for that purpose they needed £2,284. During 1908, 648 in-patients were dealt with, whilst the out-patients numbered 19,437.

OPENING OF THE WINTER SESSION IN THE MEDICAL SCHOOLS.

Nearly all the medical schools in London made a formal commencement of work for the winter session last Friday; the exceptions, in common with most of the medical faculties in the provinces, made the opening of the winter session coincide with the commencement of the present week. As has been the case for several years past, introductory addresses were not numerous, but the good old custom of marking the commencement of a new *annus medicus* by gatherings of old students round the dining table shows no diminution in popularity. Nine of the London schools held such dinners on Friday last, and two on the first days of this week. Of these and of such other ceremonies as took place some account follows. We give pride of place this year to the London School of Medicine for Women.

THE LONDON SCHOOL OF MEDICINE FOR WOMEN.

The opening of the winter term of the School of Medicine for Women (Royal Free Hospital) brought together a very large gathering at its head quarters in Hunter Street, Brunswick Square. It was indeed so numerous that the hall in which the formal ceremonies were in progress was full to overflowing, and a large proportion of the visitors spent their time in exploring the excellent theatres and laboratories which the school now possesses, and in tea and talk. Ladies naturally predominated, and a large proportion of the women doctors who have obtained eminence were present, but there was also a fair sprinkling of their male colleagues, among these being Mr. H. T. Butlin, the President of the Royal College of Surgeons, and Sir Clifford Allbutt, as well as Dr. Sainsbury, Mr. James Berry, and others immediately connected with the school. In the report of the Council, allusion was made to the recent widening of the field of work for medical women by the institution of compulsory medical inspection of school children, and it was noted that several old students had obtained good posts in this connexion. An appeal was also made to all friends of the higher education of women for help in raising a sum of £4,000 in order to free the school from a building debt which gravely hampers its further development. Mrs. Garrett Anderson, M.D., who occupied the chair, added some account of the work of old and present students of the school during the past year. Apart from minor successes, as many as seven students of the school had presented themselves for the M.D. degree of the London University, and all had passed—a somewhat remarkable achievement in view of the comparative rarity with which this rank is attained. The school had also been honoured by the admission of one of its students to the membership of the Royal College of Physicians of London. Speaking of the future, she noted the number of new students showed a very material increase, and rightly pointed out that the prospects and surroundings of those who become students of the London School of Medicine for Women were infinitely superior to those of women who in ones, or twos, or threes, entered schools primarily intended for male students; such women had to live lives of extreme seclusion, and their student career was of a much less kindling and stirring character than that of women at the school in whose halls they were now assembled. In addition to other advantages, the latter had the practice of two hospitals in its immediate proximity entirely given up to them, and the possibility before them of having responsible posts in those hospitals allotted to them. Such opportunities rarely presented themselves to women who joined men's schools. The formal address of the day was delivered by Mrs. Henry Fawcett, LL.D., who chose "pioneering" as her subject, because every one, she said, including herself, was too apt to step in and enjoy what had been won for them by the labours of their predecessors, without a thought of the years of courage and self-sacrifice which had been necessary to attain it, and as if it had been bestowed as the bounteous gift of Providence. The enthusiasm and genius of the pioneer had been sung by several modern poets, particularly Walt Whitman and

Mr. Rudyard Kipling. The latter had shown in his poem *The Explorer* how keenly he had entered into the spirit of the Pioneer. The vision, the compelling necessity, the sacrifice came to one man in ten million; he moved forward and showed the way, then the mass followed and enjoyed the fruits of his labours, and hardly gave a thought to him who had put these advantages within its reach. This was in a sense quite wholesome and natural; just as a healthy person never thought about his health, so those who enjoyed what others had won seldom thought how those benefits were obtained. Still there was a certain ungraciousness and ingratitude in accepting everything without any sort of acknowledgement. There was only one really valid excuse, and that was ignorance. But after all, complete as the excuse was, it was one of which it was scarcely possible to be proud. The speaker then made brief allusion to some women pioneers of medicine—Mrs. Anderson, Dr. Sophia Jex-Blake, Dr. Edith Pechey, and others—but dwelt at chief length on the work done in the same direction some fifteen years earlier by another Englishwoman, resident in the United States, Dr. Elizabeth Blackwell, who was still happily to be numbered among medical women of the present day. Innumerable difficulties had stood in her path, and she showed indomitable spirit in overcoming them; she took deep interest in the moral and religious aspects of her professional work, and womanliness was an essential part of her character. It was characteristic of her great tenacity that she never lost her English nationality, though for forty years of her life she was resident in the United States. The students of the present day could scarcely have a more interesting and charming work in their hands than Dr. Blackwell's autobiography, called *Pioneer Work in Opening the Medical Profession to Women*, published by Mr. Barry of Hastings. In conclusion, Mrs. Fawcett said that the students she then saw before her would go on their several paths out into the world, and although they themselves would know that they were not pioneers—that the real pioneering had been done for them by their predecessors—yet they might very probably find themselves in positions where the qualities of a pioneer would not be misplaced, and in this case one hoped that something of the pioneer spirit might be vouchsafed to them.

ST. BARTHOLOMEW'S HOSPITAL.

"Well, there's another of them over, my twentieth, and you?" "Only once to a dinner, but to half a dozen festas in the hall." These two brief sentences exchanged between old friends journeying westward last Friday night, put in a nutshell the peculiar advantage enjoyed by old St. Bartholomew's men. Old students of other hospitals have the pleasure of seeing old friends, but they meet at a restaurant, or some other ordinary caravansary. St. Bartholomew's men assemble in a hall familiar even to the newly qualified men as a place of pleasant gatherings, and they enter it as of right, as men who assemble once more in the family dining-room. Moreover, the Great Hall of St. Bartholomew's Hospital—one of the finest examples of city halls in existence—is in itself reminiscent of a successful past, and its decorations, from the Hogarth frescoes on the stairs to the paintings of past heroes of medicine between the windows, are all in keeping with the spirit of an old students' dinner. This is the reason, perhaps, why one detects an elusive difference between old students' meetings at St. Bartholomew's and those of other medical schools. There is equal pomp, even greater stateliness, but with it all an old-world flavour, a lack of personal ceremony, a resemblance to a meeting of brothers and sisters rather than of cousins, aunts, and law relations. In any case, old students' dinners at St. Bartholomew's are always a great success, and the dinner last Friday was no exception. Dr. Herringham, who was in the chair, found himself faced by quite as many old friends as the hall, which is no small one, could hold. Dealing very briefly with the loyal toasts, he proposed "The Hospital and Medical School." Since he first entered its doors the face of medicine had greatly altered. Pathology, with its subdivisions, as now understood, had practically come into existence in the past thirty-two years. What was known of the subject in his student days was taught by one or two men who likewise taught other subjects; now the school staff included nine specialists who taught that subject alone. Another point of interest was the complete change in surgery; at the beginning of that same period modern surgery was then completely in its infancy, and in 1882 Sir William Savory fought his famous rearguard fight in favour of the open wound. Dr. Herringham concluded by an expression of the gratitude of the school towards the late treasurer, Lord Ludlow, and prophesied that Lord Sandhurst would justify an equally warm feeling towards himself. It was the first dinner held since he became treasurer, the first time that he met his constituents, so perhaps he would tell them as between friends—well, what the House of Lords intended to do with the Finance Bill. Lord Sandhurst, in his reply, denied the soft impeachment; he had no notion what the House of Lords would do, but was quite certain that he himself would do his best to justify the goodwill with which he had been received. It had been a great pleasure to him to find what magnificently good relations existed between the governing body of the hospital and the medical staff, and he had voiced some of that feeling in a letter which he had recently written to Mr. Harrison Cripps, expressive of the regret alike of the governors and of the school at his resignation of the post of senior surgeon. The toast of "The Visitors" was proposed by Dr. Norman Moore, who in the course of his remarks applied a somewhat interesting simile to the older universities and that of London. Oxford and Cambridge were noble oaks affording shade and protection to every one, but London University, though, like a eucalyptus tree, it had shot up far above the heads of its compeers, yet put out leaves which somehow failed to fulfil their expected function. The reply came from Mr. Butlin, who was present not in his capacity of old student, but as President of the Royal College of Surgeons. He was, he remarked, the first President of the Royal College of Surgeons that the school had had for a good many years, but that was the fault neither of the school nor of the council of the college. Had they been ready to accept the duties of the office, both Sir Thomas Smith, whose grave illness they were all regretting, and Mr. Alfred Willett might, he was aware, have been presidents of the college in the interval. He also referred to the circumstances which had brought about his resignation, some years ago, of his position on the active staff of the hospital, a step which had caused him inexpressible regret. It was, however, unavoidable, for he was not in very good health at the time, and had he kept up his work at the hospital he would have had to abandon that which he was doing as treasurer of the fund for the creation of a combined institute for the teaching of the preliminary sciences. That scheme had finally been abandoned, but he still believed that it was the best way of bringing back the halcyon days of the London medical schools of twenty-five years ago. A reply also came from Professor Hill, who said that the driving power of London University did not come, as sometimes supposed, from its Senate, but from the recognized teachers of the university. Those who formed the Faculty of Medicine numbered some 400, and when they were all agreed as to what was wanted the Senate, he thought, would be very glad to try to meet their views. A toast to the chairman and to the secretary of the dinner, Mr. H. J. Waring, followed, and the company then adjourned to explore the new laboratory block.

CHARING CROSS HOSPITAL.

The ceremonies connected with the opening of the winter session at Charing Cross Hospital took place on Monday, October 4th, the principal feature of the day being, as usual, a prize distribution, and the reading of the Dean's report on the school work during the year. The awards were to have been distributed to the successful students by the Chairman of the Hospital, Viscount Ridley, who was also to have delivered an address, but he being unavoidably absent, Sir Clifford Allbutt, Regius Professor of Medicine at Cambridge, kindly stepped into the breach and handed the prizes to the various students. He did not deliver anything in the nature of a formal oration, but nevertheless let drop some interesting observations. It was pleasant, he said, to see the cordial way in which those who were unsuccessful as competitors for prizes greeted those who were more fortunate. It was the best

preparation for life to submit to a certain amount of disappointment, and to do so in a buoyant way without jealousy. That characteristic was, he thought, largely gained by taking part in British games. Through them they learnt the proper spirit in which to take little rebuffs and pains, to subordinate individual opinions to the ruling of a captain without recrimination, without hesitation. Continuing the same theme, he pointed out that if athletics were associated by the student with his other studies, he would enter life with the best spirit in which to take the ups and downs of fortune; but they should be players of games, not merely watchers of them. Of the study of anatomy he remarked that it was a disciplinary subject of the utmost importance, and one which he greatly favoured. He further indicated a belief that medicine in one sense was merely at the beginning of its career, since he seemed to foresee a time when the making and administration of arrangements for the application of the science and art of medicine to the direct benefit of the population in every way would be regarded as no less a duty of the State than its utilization of any other form of knowledge for the common good.

The report of Mr. F. C. Wallis, surgeon to the hospital, and Dean of the medical school, in addition to ordinary matter, contained an important expression of views with regard to the University of London and its influence on the medical schools of London. The representatives of all the London medical schools had, he said, sent a joint letter to the Prime Minister, urging that the new Royal Commission on the University "should be allowed to take evidence as to the means by which the medical degrees of the university might be made more accessible to London students." This, Mr. Wallis said, was a matter of grave, even of vital, interest to the London medical schools, and if the public could only appreciate what it meant, a lively and profitable interest in this most important matter would be taken. At any other teaching school in the United Kingdom, outside this metropolis, a student could obtain a doctor's degree for the same amount of time and money that it took a London student to get the double qualification, which, although an excellent one, did not carry with it the all-important title of "doctor." It might be said that it did not matter what a man's qualifications were if the man himself was good. But this was not true in practice, especially when a man was starting his career. In common with many others, the speaker had received various letters from former students complaining bitterly of the fact that they were surrounded by men with doctor's qualifications obtained at either a provincial, Scottish, or Irish university, at no greater trouble and expense than it had taken the London man to obtain his double qualification, and moreover, it had actually occurred that in advertising for candidates for posts at small provincial hospitals it had been stated that a doctor's degree was essential, thus effectively barring the London man with the double qualification. There was no doubt that the actual medical examinations for the London M.D. degree did not present any unusual difficulties to the ordinary examinee. The whole trouble lay with the matriculation examination, concerning which he could say much if the present were the time and place to do so. The fact, however, was known to all of them, and would, he hoped, be dealt with by the proper people when the evidence was taken at the Royal Commission. The practical outcome, as it at present stood, was that old students of the hospital said—and no one could blame them—that, much as they liked their old school, and although they knew of the many advantages in other ways which a student got in London, still, this stumbling-block of a doctor's degree was so great that they could neither send their own sons nor those of their friends to London, but recommended them to some provincial school where the facilities for obtaining a doctor's degree were made as easy as they were made impossible in London. The speaker said, made impossible, for that was what had been going on for years with regard to the London University, which, in this matter, had been held in subjection by a small body of men whom one would have credited with larger minds and greater foresight than they seemed to possess. Instead of this, with a narrow and selfish outlook they clung to all the old difficulties of the London M.D. degree, which had always made it a nightmare to the ordinary London medical student, and was now rapidly ruining London as a teaching medical centre. All the splendid wealth of clinical material, all the facilities of obtaining the best medical education in the world, were thus being lost, ruined, and thrown away, because of the selfishness of a few and the apathy of the many. These things had been frequently written about in the medical press, but it was vitally necessary for the public to take up this matter, and to do its best to protect its own interests by helping the medical schools of London to have the power to offer to the London medical student a degree which could be obtained with equal facility to any of the other degrees already mentioned. By this, and this alone, would London regain her lost position as the best medical school in the world. At a later hour the old and present students of the school assembled at dinner at the Criterion Restaurant. Dr. William Hunter occupied the chair, and, in moving the toast of the evening, included in his remarks a history of the school and hospital. The toast to the guests was coupled by Dr. Mott with the name of Sir Clifford Allbutt.

KING'S COLLEGE HOSPITAL.

Term commenced at the Medical School of King's College Hospital on October 1st, but this year the occasion was marked by nothing in the way of an address or other formal ceremony. The annual dinner of past and present students, however, took place as usual, and the Waldorf Hotel in Aldwych was again selected as the place of meeting. The arrangements, in short, so far as the dinner was concerned, were on all-fours with those which last year proved so agreeable to all concerned. The chair was taken this year by Mr. T. F. Hugh Smith, now of Farningham, but an old student of the school and a former member of the staff of the Medical Faculty of King's College and surgical registrar of the hospital. He found himself supported by the following members of the consulting and active staff: Dr. Burney Yeo, Dr. Ferrier, Professor W. Rose, Sir W. Cheyne, Mr. Barrow, Dr. Dalton, Mr. Carless, Dr. Pritchard, Drs. Silk, Phillips, Whitfield, Playfair, Flux, Steen, and Priscoe, Mr. Burghard, and Mr. Cargill. The rest of the company numbered about one hundred, amongst them being Drs. Walters, Allfrey, Short, Lewis, Shuttleworth, Child, Essery, E. Wood, Matthews, Davidson, E. Thurston, C.I.E., and the secretaries, Dr. StClair Thomson, Mr. Peyton Beale, Dean of the school, and Dr. Wiltshire. In proposing the toast of the evening, "King's College Hospital and Medical School," the Chairman pointed out the immense amount of work which the student of the present day had to get through in comparison with that of his predecessors, and also how much more strenuous a medical student's career had become. He also reminded his audience that this was the first year of the school's existence as the medical school of King's College Hospital; previously it had been part of the Medical Faculty of King's College, London, but owing to the incorporation of that body with the University of London the medical school of the hospital, though still a school of the university, became independent. This was also necessary in view of the impending removal of the hospital to South London. Dr. Walters, who replied on behalf of the school, said that his memory of it went back a very long time, for he had been one of its students in the year 1853. He gave some idea of the work of the hospital and its staff at that time, and described Farre, Todd, and Partridge as great teachers. He had no doubt whatever that when the hospital was rebuilt at Camberwell its work would be better than it had ever been; its special departments were at present hopelessly cramped, and when they were able to expand, as would be the case on the new site, the hospital, he felt sure, would come to occupy a premier position amongst London hospitals and medical schools. The evening ended with a vote of thanks to the dinner committee and secretaries, whose arrangements had resulted, it was agreed, in a most pleasant and successful evening.

ST. GEORGE'S HOSPITAL.

The commencement of the winter session on October 1st was marked, as usual, by a meeting of the St. George's Club, an old students' dinner, and an address. The latter, delivered by Dr. H. D. Rolleston, senior physician of the hospital, in the afternoon, in the presence of a large gathering, was devoted to an interesting review of the part

which St. George's Hospital and its medical school had taken in the progress of physic as apart from surgery. He commenced by an allusion to some deaths among its alumni which have taken place comparatively recently—Sir Henry Pitman, Dr. Arthur Gamgee, and Lawrence Read, one of the school's most loyal friends and oldest students, who practised for more than fifty years in Gloucester Road, not far from the hospital, and was a model of a general practitioner. Turning to the more specific subject of his address, Dr. Rolleston gave brief accounts of the careers of a number of notable men who had held office as physicians to the hospital in the distant past. Among them were Jenner of vaccine fame, Matthew Baillie, the first author of any book on morbid anatomy, who, after a career of marked scientific and financial success, achieved a monument in Westminster Abbey; and Thomas Young, the most comprehensive genius and the greatest man of science that the profession of medicine had ever possessed. He was an example of the old adage that "a prophet is not without honour save in his own country," for it was not until the greatness of his conception concerning light waves had been realized abroad that his genius was recognized in England. His career as a physician was interesting in connexion with the question of how far the possession of really great abilities and even genius bears on the acquisition of worldly success in the practice of medicine. Dr. W. H. Dickinson, who had analysed his methods of dealing with his hospital patients, said they appeared to have been less meddlesome but more successful than those of some of his more venturesome colleagues. The public, however, judged medical men more by their methods than by their results, and he did not appear to have been popular with students, or to have been financially successful in private practice. A number of other physicians of more recent date were also mentioned by the orator, and in connexion with them he reminded the audience of the observation that there was no profession in which a man might be so distinguished in his lifetime, and leave behind so slight a record of his life as in medicine. Speaking of particular diseases and special forms of treatment, knowledge of which was in a measure due to the work of old St. George's men, he mentioned that in 1821 the Royal Humane Society gave up the use of bellows as a means of restoring the apparently drowned at the instance of Sir Benjamin Brodie. Some fifty years ago Alexander Wood, of Edinburgh, commenced the injection of morphine for the relief of neuralgia, but the injections were always made precisely over the painful part. It was in the wards of St. George's Hospital that neuralgia was first treated by subcutaneous injection at a distance from the seat of pain and hypodermic medication employed for other than neuralgic affections. This advance was initiated by Mr. Charles Hunter, at that time house-surgeon to the hospital, and even as late as 1866 the efficiency of distant injections was strongly contested. Some three years later Sir Clifford Allbutt, also a St. George's man, showed the good effect of what was then an unknown treatment—namely, the injections of morphine in bad cases of heart disease. Finally, the speaker showed that St. George's men had influenced the progress of medicine in a manner often overlooked, but none the less important, by the editorial control of medical journals. Roderick Macleod, among those whom he mentioned in this connexion, was the first editor of the *Medical and Physical Journal*, a paper which Dr. Sprigge, also a St. George's man, who now edited the *Lancet* with such eminent success, described in his "Life of Wakley" as "a decent monthly magazine of some ninety pages with no particular reason for its existence." At that time Wakley was vigorously attacking abuses in the medical world in the pages of the *Lancet*, and the two journals came into opposition, the strained relations eventually landing the rival editors in the Law Courts. In 1827, Macleod left the *Medical and Physical Journal* to direct the *Medical Gazette*, which was started by some leading hospital surgeons, including Brodie and Abernethy, to counteract the "evil influences" of the *Lancet*, but it was not very successful in the fight against Wakley and his energetic methods. Another St. George's physician and editor was John Ogle, who held office for a time at the head of the *British and Foreign Medico-Chirurgical Review*. It was a journal conducted on much the same lines as the *Edinburgh Quarterly*, and probably marked the highest level of medical journalism in this country. The annual dinner of old students took place the same evening at Princes' Restaurant, under the chairmanship of Mr. MARMADUKE SHEILD.

THE MIDDLESEX HOSPITAL.

The authorities of Middlesex Hospital and its Medical School contrived this year, as on several previous occasions, to make a very popular festival out of the First of October proceedings. They included a general visitation of the wards, tea in the nurses' refectory, an address, presentation of prizes and medals both to students and nurses, and the reception of the principal guests, Lieutenant and Mrs. Shackleton, by Lord Cheylesmore and Prince Francis of Teck, Chairman and Vice-President of the hospital, by leading members of the medical and surgical staff, and by a guard of honour furnished by the hospital contingent of the Officers' Training Corps. The address came from Dr. STRICKLAND GOODALL, Lecturer on Physiology in the school, who described the evolution of a medical man at the present date. He had an appreciative audience, which promptly punctuated by laughter the touches of quiet satire by which the account was lightened. Doctors, in his opinion, resembled poets in that they were born, not made; but unlike the latter, and fortunately for the public, they had to undergo a definite course of study known by the laity as "walking the hospitals." It was a mystery which, in the laity's view, lasted a varying number of years, cost parents an indefinite amount of money, and was supposed to be associated with much experimentation on the hapless poor. At the present time, under favourable conditions (such as obtained at Middlesex) the process of walking the hospital took about five years, this being the *minimum* time allowed by law. At other hospitals—and even at Middlesex occasionally—it had been known to take longer—the long distance (time) record at present being, he understood, held by a gentleman who had up to now devoted twenty-two years of his life to it, without either tiring of the amusement, ruining his health from over-study, or satisfying the examiners! In the case of the London University medical curriculum, five and a half years was the *minimum* time allowed, the extra half-year having recently been added, possibly to enable the teachers and the students to master the regulations. During these nominal five or five and a half years the student had to acquire knowledge of no less than twenty-eight subjects and to pass a variety of examinations in them. The examiners demanded that the knowledge shown should be "sound," but each teacher expected the student to obtain an expert knowledge of his own particular subject and to devote the whole of his time to it. Giving an account of the subject of each year's work, he remarked of the preliminary sciences that the main object of their study was to enable the student to understand the later subjects. It was the broad principles underlying these preliminary sciences and their bearing on the later subjects that the student should be taught to realize. A student who began a medical career burning with a desire to "doctor people," was apt to have his enthusiasm damped when asked to retire behind a bench and occupy himself with test tubes and the like, unless the essential importance of this part of his work on his future life was constantly brought home to him by the fashion in which his teachers instructed him. Anatomy was a more popular subject with the average student than physiology, partly because its bearing on his future work was obvious, partly because he did the work largely himself. In physiology the instruction necessarily dealt somewhat largely with theory, the demonstrations were based on lower animals, and the student took a less active part in the study, and had more difficulty in realizing what use it would be to him. Moreover, it might be said that in the London schools physiology was at present less well taught as a rule than anatomy. It would be much better if all teachers of physiology in London hospitals would constantly bear in mind that they were teaching physiology to men who were going to be doctors, and not physiologists, and if they had the strength of mind to eschew the more purely academic parts of the subject and sufficient moral courage and self-denial to abandon for ever Pfluger's law, electrotonus, and the veratrine curve, and to substitute for them work in which the student was

properly taught the systematic application of physiological methods to the investigation of the functions of the different organs and systems of the human body. In that case physiology would occupy a position in the systematic practice of medicine and of treatment, and enjoy a popularity with the student and the practitioner such as it never had had and never would have under existing conditions. One great danger of specialization was loss of judgement and perspective in relation to other subjects, and there had been in the past rather too much specialization in anatomy and physiology. In the case of the teacher of anatomy an up-to-date knowledge of surgery and in the case of the teacher of physiology in medical schools a correspondingly modern knowledge of medicine was essential if they were to teach useful anatomy and useful physiology to medical students. Dealing with later subjects, he remarked that successful doctoring demanded, besides other knowledge, knowledge of patients themselves and their individual peculiarities. The importance of a psychic factor was well recognized in ordinary physiological processes, such, for instance, as digestion, and as regards disease it was, from the patient's point of view, commonly the worst feature in every case. In the case, for instance, of a simple fracture of the leg in a busy man, the mental worry caused by enforced rest and the possible consequences to his business was often a cause of much greater disturbance than the pain of the actual injury. If this were correct, the psychic factor was one to be recognized and treated just as much as the actual pathological condition. The danger of this belief lay in the fact that one might exaggerate its importance, and so overtreat it to the neglect of the actual physical factor, but since nothing was so easy to provoke as mental pain, and nothing so difficult to alleviate, ignoring the psychic factor might in the case of a sensitive patient amount to actual brutality. On the other hand, if the psychic factor were overtreated, the doctor himself became a quack, and risked making his patient a chronic neurasthenic. In the wards of the hospital the students would have an opportunity of studying the methods and manners of some of the most successful and eminent medical men, and by analysing the diagnostic skill of one, the operative ability of another, the kindly tact and common sense of a third, the academic knowledge of a fourth, they would be able to arrive at some conclusion as to what had made them famous; it was not solely a knowledge of their professional work. His concluding advice was, "Do not make friends of your patients or patients of your friends." Dr. H. Campbell Thomson, the Dean, then read the report of the school for the year, after which Lieutenant Shackleton distributed the prizes gained by the students and Mrs. Shackleton the Fardon Memorial medals for nurses. Finally Lieutenant Shackleton made a short speech. Dr. Goodall had said that their joint inexperience, the one in delivering introductory addresses, the other in distributing prizes, made them comrades in distress; but, whereas, Dr. Goodall had acquitted himself well, the speaker personally had never been nearer prizes than when he gave them away at his old school the other day. Those who were explorers did not go in much for theories, but contented themselves with finding out what things happened, and left others to find out what they meant. Among his companions on his expedition he had had three medical men, and several points of practical interest such as could not always be discovered in a laboratory had been noted. Among them was the fact that occasionally their bodily temperatures ran down far below normal—as low as 93° and 94° Fahrenheit. Another was a repetition of the experience of the Nansen expedition, that they remained quite free from nasal catarrh until a bale of clothing from home was opened. Those recovered most quickly who remained the shorter time under cover. It was also observed that rotifers, which were exceedingly fertile in hot climates, flourished equally well when the temperature was even below zero.

At the annual dinner the same evening, Mr. John Murray, who was in the chair, took charge himself of the toast to "Middlesex Hospital and Medical School." They had all, he said, heard with pleasure that day of the re-election of Sir Richard Douglas Powell, as President of the Royal College of Physicians of London; and all Middlesex men were delighted at the conferment of a baronetcy on their senior consulting surgeon, Sir Henry Morris. More patients had passed through the wards than in any previous year, and this they would all admit was in a large measure due to the energy and devotion of their resident medical officer, Mr. E. Johnson. They had only to look at the *Archives of Middlesex Hospital* to see the work which was being carried on in the cancer department under the direction of Dr. Lazarus-Barlow. The munificent Barnato bequest would facilitate its material increase. The question of an athletic ground for the school had been happily settled. In conjunction with St. Mary's Hospital it had been agreed to purchase a piece of ground at Acton at a sum of £4,000, including buildings, and it was estimated another £1,000 would be required to put the ground in proper order; the school's share of the expense would be about £2,500, and towards that sum Mr. Bland-Sutton had given a generous donation of £1,000. Lord Cheylesmore, one of those who responded to the toast, said he regarded the medical school as an integral part of the hospital; if any disaster overtook it the hospital would be equally affected. He urged more students to join the Officers' Training Corps, and thus enable the Middlesex Hospital to have its own company, instead of being combined with another hospital. The other toasts included one to "The Visitors," which, proposed by Mr. Andrew Clark, was responded to by Mr. Chevalier Ganz, who, as has been the case for many years past, supplied the diners with some excellent music. There was also an impromptu toast to Mr. Bland-Sutton. It was proposed by H.S.H. Prince Francis of Teck, who said that he had been asked by the students of the medical school to propose it in recognition of Mr. Bland-Sutton's generous aid in the matter of the athletic club. It was received with much applause, and after its brief acknowledgement by Mr. Bland-Sutton, and a toast to the Chairman, the formal proceedings came to an end.

UNIVERSITY COLLEGE HOSPITAL.

The opening of the winter session at the medical school of University College Hospital took place on October 1st, the event being celebrated by a dinner and a distribution of the scholarships lately competed for by new entrants and of the prizes awarded to existing students of the school for successful work during the past sessions. The proceedings commenced shortly after three o'clock in the library of the new school buildings in Gower Street, alongside the almost equally new hospital, and facing the old familiar frontage of the parent institution, University College, and terminated, so far as the afternoon was concerned, in tea and exchange of news in the board room, and a thorough exploration of the arrangements of the new building by those who had not had an opportunity of examining them before. Meanwhile, Dr. Batty Shaw, the Dean, gave a very satisfactory report of the history of the school during the past year, and the prizes were distributed by Sir John Tweedy, late President of the Royal College of Surgeons, and Emeritus Professor of Ophthalmology at University College. A notable point in connexion with this part of the proceedings was the remarkable success of Mr. C. E. Shattock, a son of the distinguished pathologist of that name. In addition to several prizes for class examinations he received both the Aitchison scholarship and the Bruce medal for proficiency in surgery and pathology. Finally, an address came from Sir John Tweedy, which will be found printed in full at page 1017 of this issue. The annual dinner of the old and present students of the school and hospital took place the same evening, but not, as last year, on the premises of the school itself. This year the Gaiety Restaurant was selected, and there some 126 diners assembled under the chairmanship of Mr. A. Pearce Gould, now senior surgeon of the Middlesex Hospital, but formerly a prominent student of University College. When a toast to the King and Royal Family had been given from the chair and duly honoured, Mr. A. E. Barker, senior surgeon to the hospital, proposed in appropriate terms the toast to "The Navy, the Army, and the Auxiliary Forces," coupling it with the name of Major C. G. Spencer, an old student of University College and now Professor of Military Surgery at the Army Medical College at Millbank. Major Spencer devoted his short and interesting speech to outlining in clear terms the leading principles of the Special Reserve Service and the fashion in which it was intended to use its members in case of need. The chairman himself took charge of the toast

of the evening, that to University College Hospital and its Medical School. In the course of his speech he gave an interesting account of the school as it was in his own time and of those who were his teachers. He spoke, too, of the recent loss which the school and hospital had sustained by the death of Dr. Radcliffe Crocker, and of the value of his work as a teacher and a leader of dermatology. The toast having been duly acclaimed was acknowledged on behalf of the school by its dean, Dr. Batty Shaw, who supplemented Mr. Gould's observations and his own report of the afternoon by some additional facts bearing on the recent past history and future prospects of the school and its students. A further item in the proceedings of a very pleasant evening was a toast to the chairman, which was proposed by Sir THOMAS BARLOW, senior physician to the hospital, and duly acknowledged by Mr. PEARCE GOULD.

ST. THOMAS'S HOSPITAL.

St. Thomas's, which, with its medical school at the southern end of Westminster Bridge, contrives by the extent of its buildings to balance even the Houses of Parliament on the other bank of the Thames, is among those medical schools which for some years past have abandoned winter session introductory addresses. This year it adhered to its rule on the point, so term commenced on Friday, October 1st, without formal ceremony of any kind, or anything to mark the event beyond the holding of the old students' dinner. This took place on the 1st of the month—not, as last year, at the Savoy Hotel, but at the Hotel Cecil, and brought together nearly 150 St. Thomas's men. The chair was taken by Mr. J. B. LAWFORD, ophthalmic surgeon to the hospital and lecturer on ophthalmology in the medical school; while the consulting physicians were represented by Dr. J. F. Payne, the consulting surgeons by Mr. B. Pitts and Mr. E. Nettleship; the physicians by Drs. S. J. Sharkey, T. D. Acland, H. B. Hawkins, and H. G. Turney; the surgeons by Messrs. C. A. Ballance and A. E. Robinson; the out-patient physicians by Drs. C. R. Box and A. E. Russell; and the out-patient surgeons by Messrs. Cuthbert Wallace, E. M. Corner, and P. W. G. Sargent. Other members of the staff present were Dr. W. W. H. Tate, obstetric physician, and Mr. J. G. Turner, the surgeon in charge of the dental department, while prominent among the guests was Professor William Osler, of Oxford. Mr. Lawford himself proposed the toast of the evening, and in doing so pointed out that although the pleasure of meeting old friends was great, their satisfaction was tempered on this occasion by a note of sadness. Several old friends had died during the year, and others whom they were accustomed to see at the annual dinners and elsewhere were on this occasion absent, and unfortunately through illness. Among the former was his immediate predecessor in the office of chairman, Mr. Wakley of the *Lancet*, and among the latter Mr. Clutton, their senior surgeon, and the popular treasurer of the hospital and strong supporter of the school, Mr. Gadesden Wainwright. He was glad, however, to note that they had among them as a hale and hearty father of the gathering Dr. L. W. Sedgwick, of Paddington, who dated his studentship at the hospital back to the year 1845. The toast was acknowledged on behalf of the hospital by one of the almoners, Mr. C. T. Harris, who was glad to be able to report that the harmony which had long characterized the relations between the medical staff of the school and the governing body of the hospital remained uninterrupted. The reply on behalf of the school came from the dean, Mr. CUTHBERT WALLACE, whose speech seemed to his many old friends pleasantly characteristic, even down to the flick with which he turned over his notes. The school, he noted, had done just as well as ever so far as examinations on the mental side were concerned, but in respect of physical exercises it had been somewhat unlucky on the whole. It did not come out as a winner at as many inter-hospital competitions as had been hoped would prove the case, but nevertheless it carried off the cricket cup, and also held the lawn tennis cup for six months, while Guy's had retained the latter for the first half of the year. The school had also proved its capacity on the golf green, and the inter-hospital championship at present stood to its credit. The whole evening passed off very pleasantly, and it was generally agreed that the honorary secretaries were much to be congratulated on the success of their arrangements.

ST. MARY'S HOSPITAL.

The ceremony by which work for the winter session was initiated at the medical school of St. Mary's Hospital on October 1st, included the reading of a report on the work of the school by its Dean, Mr. Clayton Greene, a prize distribution, and the delivery of an address. Both the latter duties were undertaken by Mr. H. A. MIERS, D.Sc., F.R.S., formerly Waynflete Professor of Mineralogy at Oxford, and since last year Principal of the University of London. He was introduced to the assembly by Dr. A. P. LUFF, who presided, and devoted the time at his disposal to a consideration, in broad outline, of systems of subjective and objective education. It was an attractive address, its main statements seeming reducible to the following propositions. Youth is the creative and inventive period of life, and though steady painstaking observation and accumulation of facts is always to be inculcated, it is a mistake—once general education has been completed—to insist too strongly on youth being treated as a time to be devoted solely to the absorption of other men's ideas, of preparation for some period of future performance. If men never undertake anything for themselves until they are fully and completely prepared for it, they will never get very far; some men become so obsessed with a notion of the necessity for preparation that they pass an existence of promise without ever attaining to performance. Education of this order, and also that which deems subjects in which pupils do not take an interest a specially useful form of mental training, lacks vitality, fails to supply that inspiration of ideas which does so much to lessen the drudgery of preliminary studies and stimulate later work. Young men should be encouraged to work out their own ideas, to form their own theories, and to be ready to abandon them when they find that facts do not fit in with them; to do all their work inspired by a restless desire to prove or demolish the theory on which it rests. Work uninspired by an idea is as aimless as a life uninspired by principles. Some modern educationalists seem to expect quite young children to take an interest in theories and first principles; but if this is a mistake it is equally erroneous to urge university students to devote the whole of their energies to provident but blind acquisition of knowledge for some unknown future use. To no one more than to the medical student who has to learn many different subjects and to pass many examinations is a spirit of research in all his work necessary. If he sometimes asks himself why he should be expected to devote so much time to chemistry, physics, and mathematics, and other subjects of no obvious utility when examination days are over, it is partly due to the narrowness of his own outlook, partly to the very zeal and excellence of the teachers of these preliminary subjects. One great fault in modern education at present is that each subject is taught on the same lines to all students alike. A mathematician teaches his subject as though all his students wished to become mathematicians, a chemist as though they wished to become chemists; and textbooks are written in the same spirit. Properly taught and properly studied in the case of medical students, these preliminary subjects ought not to be chemistry or physics or mathematics, but the chemical or physical or mathematical aspects of medicine and illustrations of the principles of that science, the materials and the tools by which its theories can be tested. Of all the branches of science with which a medical student is expected to be acquainted, including mathematics, there is none which cannot be taught and made interesting to him through his own special subject—the art and science of medicine to which he is eventually to devote himself entirely. It would no doubt be very satisfactory if the evolution of every medical man involved his being first a complete mathematician, then a complete physicist, then a thorough chemist, and so on through different stages; but life is not long enough for such a process, and the minds of few men are capable of containing so much knowledge. If it be objected that no one can form theories until he has a complete knowledge of facts, the reply is that every student or other person who endeavours to understand an experiment done

in his presence before its working is explained to him, or to grasp the why and wherefore of any fact or circumstance that he observes in his daily life, is in fact inventing his own explanation, or, in other words, supplying his own theory. The views put forward were illustrated and supported by references to known facts in the careers of several bygone masters of science. The day ended in a pleasant gathering of old and present students at the Café Royal, Dr. Milner Moore taking the chair.

THE LONDON HOSPITAL.

As has been the case during the past few years, the medical school carried on in connexion with the London Hospital dispensed with the formality of an address introductory to the work of the winter session, and nominally commenced work forthwith on October 1st. The day, however, was not allowed to go unmarked, for, as last year, dinner-gathering of old students was held in the evening and in the afternoon the Staff was "at home" to all old friends at the college. The old students' annual dinner used likewise to take place at the college, but more recently has been held in some restaurant. Last year the Savoy Hotel was selected for this purpose, and it was there that about 160 old students assembled last Friday evening under the chairmanship of Dr. F. T. Smith, one of the senior physicians. The toast list was commendably short, for, apart from the usual "Health to the King" and another to "The Patroness of the Hospital, Her Majesty the Queen," whose active interest in the institution is so unfailing and so beneficial, there was only one item, the toast to "London Hospital and its Medical College." This was given from the chair by Dr. Smith, who in the course of his remarks expressed the regret alike of the staff, the old students, and the school at the death of Sir Stephen Mackenzie. He also referred in corresponding terms to the retirement from active connexion with the school and hospital of Mr. Charles Mansell-Moullin, late Senior Surgeon. Another old-time and familiar member of the staff whose name was brought forward prominently was that of Sir Jonathan Hutchinson—the official recognition, by the conferment of a knighthood, of his great abilities, untiring scientific industry, and constant endeavours to promote the progress of the art of medicine and surgery had been warmly welcomed by his old colleagues at the hospital, of which he was now Senior Consulting Surgeon, and by the staff and pupils of the school with which he had been so long directly and indirectly connected. Another subject to which the Chairman drew attention was the work on the gastroscope recently done by two old students of the school—Dr. H. Theodore Thompson, now Assistant Physician and Pathologist, and Dr. H. S. Souttar, until recently a House-Surgeon. This important research had been materially aided by a grant of money from the Research Fund lately founded at the hospital through the generosity of an enlightened but anonymous donor who gave £20,000 for the purpose. In conclusion, he drew attention to the Masonic Lodge which had been founded in connexion with the school and hospital, and whose existence, he thought, did not seem to be as yet sufficiently known to old London men.

[Several other reports which reached us too late for insertion in this issue will be published next week.]

LITERARY NOTES.

Messrs. Longmans and Co. will shortly publish a work, entitled *The Relations of Medicine to Philosophy*, by R. O. Moon, M.A., M.D.Oxon., F.R.C.P. It is described as an attempt to trace the influence exercised upon medicine by the current thought and philosophy of the day. Beginning with the pre-Socratic philosophers, various important epochs in the world's history have been selected, showing the varying relations of medicine and philosophy. Thus some periods, such as those identified with Bacon and Descartes, have been strikingly favourable to the progress of medicine; others, like the Middle Ages, markedly the reverse. In particular, Dr. Moon deals with the influence of early Christianity, which he says has been favourable to medicine viewed as an art, though inimical to its progress as a science. The German philosophers of the latter part of the eighteenth century are represented as having enlarged the intellectual horizon of medicine, while the tendency in more modern times for medicine to become isolated from philosophy is dwelt upon. Special chapters are devoted to Galen, Arab medicine, and Paracelsus.

The October number of *Travel and Exploration* (the first under the new editorship) covers a fairly wide field, ranging from the Far North to the Far East. Especially noteworthy are the narratives of two of the greatest explorers of the day in their respective fields, Dr. M. Aurel Stein and Knud Rasmussen. Dr. Stein describes a remarkable journey through the arid wastes of Chinese Turkestan, across the "Sea of Sand" (the picturesque native sobriquet for the Taklamakan Desert). Knud Rasmussen, the greatest living authority on the Esquimos, writes a brilliant little impressionist sketch describing a long journey on ski with a Lapp caravan. Passing from the Frozen North to the luxuriant tropics, the reader is introduced to the ex-Queen of Java by Miss Scheltema, a Dutch authoress, who is a keen observer and an experienced traveller. Readable accounts of travel and experiences in Palestine and Cyprus are furnished by Mr. F. G. Aflalo and Mr. Justice Bertram. Finally, Mr. Elliott Stock describes very vividly a dangerous climb of the Great Gable "chimney" in the English Alps region, an article which will appeal to mountaineers in general and rock climbers in particular.

A doubt has always hung over the death of Jean Jacques Rousseau. It has been said, among others by Madame de Stael, Villemain, Grimm, and many others, that he committed suicide. There is nothing intrinsically improbable in this, as it is certain that the Citizen of Geneva was mad in the later years of his life. In 1866 Dr. Dufour of Amiens, Perpetual Secretary of the Académie de Médecine, treated the fact of suicide as proved, and since that time it has been generally accepted. The question has, as we learn from an article by Dr. J. Noir which appears in the *Progrès médical* of September 25th, recently been fully discussed by Dr. Fabien Girardot in a work entitled *La Mort de Jean Jacques Rousseau*, published by Maloine of Paris. He passes all the available evidence in review. He holds that Madame de Stael's hypothesis that Rousseau made away with himself on discovering the infidelity of Thérèse Levasseur is untenable, as Jean Jacques's feeling for her had become one of such indifference as to exclude jealousy. Grimm believed that Rousseau was driven to suicide by the fear that his *Confessions* would be published prematurely—a conjecture still more improbable, as he evidently took a pride in revealing himself to his fellow men. Dubois thought that the cause was insanity. There is, however, the evidence of his doctor, Le Bègue de Presle, that in his very last days Rousseau found at Ermenonville a serenity and peace of mind which seemed to warrant the hope that he would live some years. The cause of death is believed by M. Girardot to have been not a pistol shot through the head, but apoplexy due to uraemia. The wound which is visible in the death mask taken by Houdon is believed to have been due to an injury received when he was struck down.

We have received a reprint of a fine address delivered at the Massachusetts General Hospital on the sixty-second anniversary of Ether Day by Professor William H. Welch, of Johns Hopkins University, Baltimore. The history of anaesthesia was told in detail in the British Medical Journal of October 17th, 1896, and there is little in Professor Welch's address that may not be found there. In the following passage, however, he does justice to the claims of Dr. Crawford W. Long as a pioneer of anaesthesia. As these have often been ignored, we quote the tribute paid by Professor Welch to his memory.

> The honour of making the first trial of anaesthetic inhalation in surgical operations belongs to Dr. Crawford W. Long, a respected and honourable country doctor, then living in Jefferson, Jackson County, Ga., who, in March, 1842, removed painlessly a small tumour from the neck of James M. Venable, anaesthetized by ether. He seems to have performed at least eight minor surgical operations during the next four years upon patients under the influence of ether. Dr. Long is necessarily deprived of the larger honour which would have been his due had he not delayed publication of his experiments with ether until several years after the universal acceptance of surgical anaesthesia. It is also to be regretted that his published details of the mode of administering the ether and the depth of the anaesthesia are so meagre and unsatisfactory. While the accepted rule that scientific discovery dates from publication is a wise one, we need not in this instance withhold from Dr. Long the credit of independent and prior experiment and

discovery, but we cannot assign to him any influence upon the historical development of our knowledge of surgical anaesthesia, or any share in the introduction to the world at large of the blessings of this matchless discovery.

Long's work, though published by himself in 1849, and more fully in 1852, was not made generally known to the profession until 1877, when he found a *vates sacer* in Marion Sims. We may be allowed here to reproduce some of Weir Mitchell's lines on the fiftieth anniversary of anaesthesia quoted by Professor Welch, although they have already appeared in the JOURNAL:

> Whatever triumphs still shall hold the mind,
> Whatever gift shall yet enrich mankind,
> Ah! here no hour shall strike through all the years,
> No hour as sweet, as when hope, doubt, and fears,
> 'Mid deepening stillness, watched one eager brain,
> With Godlike will decree the death of pain.

Authors often find fault with their illustrators for obvious mistakes, as for instance, for giving a character two right feet, or for glaringly misconceiving or misrepresenting the text. But writers themselves are not exempt from human frailty in this matter. Our English writers are often enough careless, but, knowing the enormous importance attached by French writers to minute accuracy of detail, we are surprised and amused by some examples gathered by Dr. Bougon and published in the last number of the *Chronique Médicale*. Thus Paul de Saint Victor tells us that the prophet Ezekiel read with one eye and wrote with the other. Alexandre Dumas in *San Felice* gives a thrilling description of an obstetrician holding between his teeth a handkerchief in which the newborn babe was wrapped, and a pistol in each hand. As if this athletic feat were not enough, he is represented as hurling himself head first into the middle of a crowd, crying out through his clenched teeth, "Room for the dead mother's child!" Ponson du Terrail describes a handsome old man as walking in his garden with his hands behind his back, reading a newspaper. Alfred de Musset speaks of the mouth keeping silence that it may listen to the heart speaking. This may perhaps be forgiven on the score of poetic licence; but what are we to say of Alexis Bouvier who, having mentioned a phial considerably earlier in the narrative, describes a wretch as throwing himself on the child, seizing its head and emptying *its* contents into its mouth? The most striking example of all is taken from Flaubert, who was nothing if not meticulously exact in his descriptions, and who yet tells us in *Madame Bovary* of some one who received as a birthday present "a fine phrenological head, all painted blue and marked with figures down to the—thorax!"

Human nature is always the same, whether arrayed in the velvet and lovelocks of the Restoration or the frock-coat and top-hat of to-day. In common with the rest of mankind, Samuel Pepys, most human of men, suffered from periodical fits of tidiness, when he turned out drawers and desks, and expended much time and energy in sorting and destroying old papers. An entry in his diary dated December 31st, 1664, reads thus:

> This Christmas I judged it fit to look over all my papers and books; and to tear all that I found either boyish or not to be worth keeping, or fit to be seen, if it should please God to take me away suddenly. Among others, I found these two or three notes, which I thought fit to keep.

Amongst these notes are the following charms:

> CHARMES.
>
> 1. *For Stenching of Blood.*
> Sanguis mane in te,
> Sicut Christus fuit in se;
> Sanguis mane in tua vena
> Sicut Christus in sua poena;
> Sanguis mane fixus,
> Sicut Christus quando fuit crucifixus.
>
> 2. *A Thorne.*
> Jesus, that was of a Virgin born,
> Was pricked both with nail and thorn;
> It neither wealed, nor belled, rankled nor boned.
> In the name of Jesus, no more shall this.
>
> 3. *A Cramp.*
> Cramp be thou faintless,
> As our Lady was sinless,
> When she bare Jesus.
>
> 4. *A Burning.*
> There came three Angells out of the East;
> The one brought fire, the other brought frost—
> Out fire; in frost—
> In the name of the Father, and Son, and Holy Ghost.
> Amen.

Pepys does not tell us where or when these charms came into his possession, or anything of their history. They are obviously of Christian origin. But long before the Christian era spells were in use for the cure of disease. So hard-headed a man as the elder Cato—he of the *atrox animus*—gives a number, and Dr. Payne in his FitzPatrick Lectures for 1903 (*English Medicine in the Anglo-Saxon Times*) cites several of Pagan origin and others in which Scriptural names, heathen mythology, and old Teutonic folklore are mixed up. Doubtless, in patients of suitable mental constitution suffering from diseases curable by faith, these charms did good in the same way as those of Christian Science do under like conditions at the present day.

Medical News.

THE first meeting for the session of the Medical Society of London will be held on Monday next, when the incoming president, Dr. Samuel West, will deliver an address at 8.30 p.m.

MR. W. A. MEREDITH, of Little Massingham Manor, Lynn, Consulting Surgeon to the Samaritan Free Hospital, London, has been placed upon the Commission of the Peace for the County of Norfolk.

PROFESSOR WILLIAM WRIGHT will read a paper before the London Hospital Medical Society on Some Problems in Teratology. The meeting will be held on Thursday, October 21st, at 8 p.m., in the anatomical theatre of the Medical College.

THE annual meeting and festival of the Birmingham Ward of the Guild of St. Luke will be held on Wednesday, October 27th, at 8 p.m., at Holy Trinity Church, Coventry, when the festival sermon will be preached by the Lord Bishop of Worcester. All medical practitioners in the district are invited to attend.

FURTHER experiments have recently been conducted by the British Fire Prevention Committee with the object of obtaining precise data as to the fire resistance of treated flannelette after many washings, and of devising some easy method of rapidly proving the precise resistance of flannelette sold as non-inflammable. An illustrated report will shortly be issued.

THE next meeting of the Society for the Study of Inebriety will be held in the rooms of the Medical Society of London, 11, Chandos Street, Cavendish Square, W., on Tuesday, October 12th, at 4 p.m., when Dr. Milne Bramwell will open a discussion on Suggestion and its Rôle in the Treatment of Inebriety.

THE first Hunterian Lecture of the Hunterian Society is to be delivered at 8.30 p.m. next Wednesday, October 13th, at the London Institute by Dr. Sidney Martin, who will discuss certain infective processes in the intestine and their results and treatment. All members of the profession are invited to attend.

IN his address next Monday, on the occasion of the opening of the winter session at the West London Post-Graduate College, Dr. Theodor Schott, of Nauheim, will deal with some further investigations on the subject of acute overstraining of the heart. Tea and coffee will be served at 4.15 p.m., and the address delivered at 5 o'clock.

MRS. SIDNEY WEBB has given notice on behalf of the National Committee to Promote the Break-up of the Poor Law that application for tickets for a meeting at St. James's Hall, London, on Tuesday next, "to inaugurate the Minority Report campaign," have been so numerous that it has been arranged to hold a second meeting on November 16th, when the Earl of Lytton will preside. Tickets for this second meeting can be obtained on application to her at 5 and 6, Clement's Inn, W.C.

A HOME for the treatment of lupus will be opened shortly in Vienna. It will be first of its kind in the Austrian Empire. No other cases will be received. The treatment will be in the hands of a special staff, the members of which are now undergoing instruction in the Lupusheilstätte of the general hospital in Vienna. Treatment will be free to all patients sent by hospitals; if a private practitioner sends a patient, a small charge will be made. The hospital will contain twenty-four beds, to accommodate surgical cases, and five sets of Finsen apparatus, together with *x*-ray apparatus. A home for patients coming from abroad will also be provided for. The expenses will be met partly by the State, partly by charitable contributions.

British Medical Journal.

SATURDAY, OCTOBER 9TH, 1909.

THE POOR LAW COMMISSION REPORT AND PROVIDENT DISPENSARY SCHEMES.

THERE seems to be some ground for fear that discussion of the Poor Law Commission report may be obscured by considerations of almost a party political character, and it is, therefore, with some regret that we observe the propagandism that is being carried on by supporters of the Minority scheme. Meetings are being organized all over the country, personal letters to Poor Law officers and to private individuals are being sent broadcast, with copies of a speech by Mrs. Webb and with leaflets advocating the Minority and attacking the Majority scheme. The slightest reflection will show how calamitous the result might be. A special committee of the British Medical Association is now considering the whole subject, and it may be relied upon to take an impartial view of every question. Meanwhile, however, it would be impossible, even if it were desirable, to prevent discussion, and it has been thought necessary to draw attention to what must be considered certain too confident statements and claims made on behalf of the Minority scheme. To allow them to pass unnoticed might, perhaps, suit certain propagandists, but it would tend to defeat the object of the special committee which is to sift thoroughly all sides of the subject. But we wish it to be definitely understood that we hold no brief for either the Majority or the Minority report. The characteristic of the Majority report is its attempt to develop individual responsibility, while the Minority aims more especially at stimulating the consciousness of social obligations. Both of these are excellent objects, and it may be said that each report is strongest where the other is weakest. That being so, why cannot we adopt the best points from each report and from the special memoranda, so far, of course, as they are not incompatible? The more this idea is considered the more we are convinced that in that direction will be found the best solution of the problems, and that a systematic eclecticism will be infinitely preferable to a slavish adhesion to either one or the other report.

Pursuing now what we claim to be an impartial examination, we come to consider certain serious points in the Majority Report, and first and foremost is the Provident Dispensary scheme. The Minority Report has rightly described in scathing terms what is called "medical demagogy," and none know so well as medical practitioners the evils of club practice, which rival in their absurd and demoralizing character the out-patient departments of many voluntary hospitals. But while all the evils of club practice must be recognized, it is one of the greatest merits of the Majority Report that it fully recognizes the need for fostering self-provision against sickness. It is by no means easy, however, to state in definite terms what the Commissioners intend. The suggested functions of the Public Assistance Authority include one thus described: "A general system "of provident dispensaries shall be organized, "of which existing voluntary outdoor medical "organizations shall be invited to form an integral "part." Mr. Loch, one of the Majority Commissioners, in addressing the Poor Law Medical Officers' Conference, said that the Majority scheme "proposed to merge provident dispensaries and sick "clubs in the Poor Law." It is true that he went on to say, what seems hardly compatible with this, that "the provident system would remain independent of "the Poor Law or public assistance, but the Public "Assistance Authority, acting on behalf of the com- "munity at large, would '*play up to*' the dispen- "saries;" and further on he said that "the division "actually in force would continue at least for some "time." From these somewhat conflicting statements no very clear idea can be got as to the exact relations that are proposed between the State and the dispensaries. There is sufficient material, however, on which to base some criticism, though it must be remembered that the Commissioners advise that the British Medical Association should be invited to suggest a scheme.

Now, at first sight, it may seem to be an attractive prospect to general practitioners that all persons below a certain wage limit, including not only the destitute paid for by the State, but the lower working classes paying for themselves, should be members of a vast provident dispensary system, the medical staff consisting of all medical men who are willing to act. It would seem to offer the chance of fewer bad debts among the working classes. But, on closer examination, its attractiveness is greatly lessened.

In the first place, there is one characteristic of the scheme of the British Medical Association for provident dispensaries which seems to be essential—namely, that its efficiency will depend on the management being kept entirely in the hands of the medical profession. That this should be so we regard as quite reasonable. Whatever may be said by some of the friendly societies, club practice must be regarded as a form of medical charity. The advantage to the patients is out of all proportion greater than the advantage to the doctors, and the difference, whatever it amounts to, is pure charity. That being so, it is not unreasonable that it should be a fundamental part of the provident dispensary scheme of the British Medical Association that medical men should keep in their own hands the entire control and management. The question then arises, Would proper control be retained if the provident dispensaries became part of a State service? The answer cannot be doubtful—the profession would lose such control.

It may be observed that the Majority Report does not propose that the dispensaries should be completely absorbed into a State service. Nevertheless, it is certain that any connexion whatever with the State, even short of complete absorption, means partial State control with various concomitants which need careful consideration. If it were simply proposed that the State should pay the subscription for only the aged and widows with young children, the only classes of persons actually mentioned, even then it must be remembered that the State cannot discharge its liabilities by simply paying the fees. The public will properly demand a right of inspection, and records of attendance and treatment will be required. That involves extra work, which must be paid for, and apart from that we must here again remember that club work is charitable work, and it may well be asked if medical men are to give their charity to the State. If not, then it would seem to follow that the fees for State patients must be higher that those paid by the

working classes. But even supposing this matter of fees satisfactorily arranged, the liabilities of the State will not yet be discharged. The question of food and medical extras still remains to introduce fresh complications. We may ourselves have a high consciousness of the integrity of medical men, but we cannot neglect the objection of the Minority Report that if there is free choice of doctor the number of State patients that any individual doctor would get would depend on the freedom with which he ordered special food, and medical extras, such as brandy, and there would be some temptation to gain popularity by laxness in condemning bad personal habits and hygiene. It is suggested that the right of choosing the doctor might be reserved for those who paid their own fees, the State patients being allotted equally among the staff; but even then the public might very probably object to medical practitioners being able to order food and extras at the expense of the State unless they were under the strict control of the State. It is useless to say that a relieving officer would see to that, for no layman dare interfere with medical orders, except by sending the patient into an institution which would often frustrate the very object of the provident dispensary.

But something far more than this is plainly foreshadowed. A time would seem to be foreseen when all the poor would be members of a provident dispensary, so that it would be possible to do away with the district medical officers. It is true that Mr. Loch thinks that in urban and populous districts this is "rather visionary," but how far it is "visionary" depends on several factors. We imagine it would be blind optimism for the State to depend on poor people voluntarily joining a provident dispensary, for it is practically certain that, with all the inducements that can be offered, a large proportion of the lowest classes could not—or, if they could, would not—join if they had to pay their own subscription. What would happen would be simply this: in some districts authorities with exaggerated ideas about the value of deterrence, would give few, if any, paupers the privileges of a provident dispensary, and the district medical officers would be retained. In other districts, however, authorities with strong ideas about the stigma of pauperism would soon enrol all their destitute in a provident dispensary, and there would be no need for the district medical officers—in fact, their fate would be as settled as it would be under the Minority scheme, where, according to Mrs. Webb, none but whole-time medical officers are intended. There are not wanting those who think that both reports intend to abolish part-time district medical officers. We do not hesitate to say that no necessity has been shown for so radical a change. If and where their work has been unsatisfactory, the fault is admitted to be due to other conditions, not to the fact that they are part-time officers.

There are dangers, too, with regard to the wage limit. A uniform limit can hardly be fixed for the whole country, owing to the different conditions, and there would be a constant struggle between authorities, who are always ready to favour the public, on the one side, and medical men, who are not always united enough to resist exploitation, on the other. We need hardly point out how disastrous to private practitioners such raising of a wage limit might be. It is, indeed, very doubtful whether the profession would not find the public authority as hard, if not a harder taskmaster than the friendly societies are now, both in this and other particulars. It is on this account that it is so important that the control of the dispensaries should remain entirely in the hands of the profession.

We do not here discuss the opposition that would certainly be raised by the friendly societies to the scheme. But looked at from the standpoint of the public, it may well be asked: Is ordinary club practice as carried on to-day suitable for a State service? That it would not be a complete service is self-evident, as it takes little or no note of the prevention of disease. But it may be argued that though prevention and cure may well be functions of one and the same authority, there should be separate individual workers for each. But even if that were granted, it is certain that neglect of hygienic and dietetic treatment, perfunctory examinations, and that treatment which consists in a bottle of medicine with a scrap of advice thrown in, ought not to be tolerated in any service managed or even subsidized by the State. Some difficulty might also arise from the fact that clubs generally refuse to treat certain diseases such as syphilis, alcoholism, etc., and also that persons who are already sick or have some constitutional weakness are excluded by an entrance examination. The ordinary arrangement for the latter cases is that an extra fee for admission is charged. This would be an enormous cost to the State as so large a proportion of persons first apply for assistance only when already sick.

We have by no means exhausted the list of problems that would have to be settled under any scheme of provident dispensaries receiving State patients. We believe the feeling of the profession is one of the strongest sympathy with the desire of the Majority Commissioners to foster self-providence. At the same time we agree with the staunchest supporter of the Minority Report that a huge system of State-conducted medical clubs on the models of the ordinary existing clubs would be a calamity both to the community and the medical profession. Provident dispensaries conducted on the lines laid down by the British Medical Association might do much good, and not least in helping to reform existing clubs; and Mr. Loch's pregnant remark that a Public Assistance Authority might well "play up to" such dispensaries opens up a vista of possibilities that no doubt will receive fullest attention from the Association. But we cannot escape from the conviction that while co-ordination and co-operation between the State and voluntary medical agencies is eminently desirable, and indeed necessary, too much dependence ought not to be placed by the State on agencies which are always variable and often unreliable both in quality and extent.

THE SERUM TEST FOR SYPHILIS.

To solve a problem involving half a dozen variables is a game not worth the candle unless the results are to prove of very great value. In the case of the serum diagnosis of syphilis it is manifest, from the enormous number of papers published since Wassermann and Detre broached the question in 1906, that the highest importance is attached to the demonstration of its feasibility. If it be found possible to anticipate the clinical recognition of recent infection, if the existence of latent disease can be demonstrated in the absence of all overt evidence, if the differential diagnosis of an obscure lesion can be completely simplified, if a reliable measure of the efficiency of treatment can be provided, if a qualitative and quantitative estimate of the virus present in any given case can be obtained—then the establishment

of therapeutic control over syphilis will be much nearer accomplishment than is that of tuberculosis, for instance.

This point has not yet been reached, but everywhere pathologists betray great energy in striving after it. Objections to the original method of Wassermann have been epitomized by Schlimpert.[1] "It is too complicated; too many controls are "necessary; too much material is required; the "results are uncertain." To this indictment may be added the drawback that the expense of its employment in private cases must be considerable.

The majority of the named modifications already in the field aim at simplification. The method of Noguchi does not come into this category, but it is worthy of remark that Howard Fox,[2] Noguchi's collaborator and a warm advocate of this particular procedure, writes: "It is only fair, however, to say "that at present the two methods give fairly parallel "results in the writer's hands."

This is not by any means the only example of an admission—only to be expected where a laboratory method is introduced that involves delicate manipulations and depends upon complex theoretical considerations—an admission that the results, obtained after considerable experience with any one method, tend to exhibit greater uniformity. This is really a hopeful sign, because there is hardly one of the variants of the Wassermann test that has not given excellent results, and yet each one has been vigorously condemned by the adherents of rival modes.

The phenomena involved are not such as to permit classification of the test as an example of the Bordet-Gengou reaction which engendered the idea, but Flashman and Butler, whose able and thoughtful paper is published at page 1019, give reasons for thinking the relations at first believed to exist have been too lightly divorced, and that appreciation of an immediate chemical basis should not exclude from consideration the biological source of the agents involved. Extracts of normal organs and other substances, not competent "antigens," prove to be competent components of the test system. Extracts of syphilitic organs may or may not be better materials than known lipoids, such as lecithin. The quantity and quality of "natural" amboceptor and complement potent in human blood for given corpuscles, may or may not be as easily estimated and controlled as is the case with artificial products. Opinions as to the rapidity of deterioration of complement differ widely. Indeed, there is no limit to the divergencies of observations on each of the details.

Another hopeful sign is that most workers can find a method which in their own hands will give a satisfactory result. Thus Fox, with the Noguchi test, obtained 87 per cent. positive results in manifest syphilis, 62 per cent. positive in cases of syphilis without manifestations, and of 23 non-syphilitic cases the only 2 which gave positive results were instances of leprosy. Leprosy and framboesia, it may be mentioned, are recognized exceptions to the specificity of the reaction; cachexia, which Schlimpert and others found to give positive results with Stern's method, and scarlet fever, which König,[3] using Hecht's modification, accused of vitiating his results, have not proved bugbears to other observers.

It would be easy to multiply examples of the thousand and one pitfalls besetting the investigator of this problem, but it is enough to say that every day advances are being made that will speedily bring the test into daily use.

No one has evinced much clearer appreciation of the factors involved or has projected a simpler *rationale* than Fleming.[4] Criticisms of his plan, which is based directly on that of Hecht, have been chiefly theoretical, but Bassett-Smith's[5] comparative examination of the method throws some doubt on its reliability. His routine requires nothing not easily obtained, and no more than an ordinary laboratory education, whilst the results afford a most striking demonstration. The records with known syphilitics and with those in whom syphilis could be excluded with some degree of probability compare favourably with those given above by Fox, and even with those of MacDonagh, Muller, and Morawetz,[6] who, however, have investigated a very much larger series of cases. Flashman and Butler now furnish statistics for the Wassermann test proper which hardly admit of improvement, but there is room for further simplification before the test can be employed to anything like the extent its usefulness demands.

[1] *Deut. med. Woch.*, August 12th, 1909, p. 1386.
[2] *The Journ. of Cut. Dis.*, August, 1909, p. 338.
[3] *Wien. klin. Woch.*, August 12th, 1909, p. 1127.
[4] *Lancet*, May 29th, 1909, p. 1512.
[5] British Medical Journal, August 14th, 1909, p. 377.
[6] *Practitioner*, September, 1909, p. 307.

THE UNIVERSITY AND THE MEDICAL SCHOOLS OF LONDON.

The first days of this month have seen the opening of another winter session, and by the time these lines appear several hundreds of young men throughout the country will be beginning to realize something of the difficulty of the work which lies before them. Meanwhile at least an equal number of old students have foregathered once more under the aegis of their *Alma Mater*, and while all have listened to after-dinner speeches, a proportion have been present at introductory addresses, which are still popular at certain schools and faculties, and if few now in number, show no falling-off in quality.

One of them, that of Sir John Tweedy, appears in full in this issue, and of others, such as those of Dr. Rolleston, Mrs. Garrett Anderson, and Principal Miers, some account will be found elsewhere in this issue. It is not, however, to these various deliverances — reminiscent, analytical, or hortatory — that we desire to direct attention in this place, but rather to a dictum in the report on the work of his school, read at Charing Cross Medical School by its Dean, Mr. F. C. Wallis. It referred to the influence of the University of London on the London medical schools, and will be found reproduced in an article on page 1079.

Both Mr. Wallis's discontent with the present and his fears for the future we are disposed to regard as well founded. To put their gist briefly, parents are proving in increasing numbers each year that they do not consider it good policy to enter their sons at medical schools in London when, by entering these same sons at Scottish, Irish or provincial schools, they can be certain that they will blossom, in due course, into degree-holders at the cost of no greater expenditure of industry, time and money, than are required to obtain the diplomas of the licensing corporations.

A university degree has a very definite value in the public mind, so that the parent's view is quite natural. It may be said, of course, that a student who begins

his medical course in London need not necessarily aim only at the diplomas of the English Conjoint Board, but in a large percentage of cases this is practically the only course open to him. It is not that the professional examinations for the medical and surgical degrees of the university are too difficult. On the contrary, now that the courses of study have been rendered less rigid and brought more into touch with the real requirements of a medical student's education, these examinations present no greater obstacle to a fairly diligent student than do the corresponding tests imposed on those seeking the Conjoint diplomas of the Royal College of Physicians and the Royal College of Surgeons. The trouble lies in the matriculation examination, which we have ventured on more than one occasion to call an antiquated barbed-wire fence. It is not that the ostensible standard in any one subject is so high that a fifth or sixth form boy who has turned his opportunities at school to good advantage ought to have cause to fear failure, but all five subjects selected have to be passed together, and this, in the opinion of public school masters, demands in the case of most boys at least some degree of special preparation. Furthermore, the examination as conducted is so tricky that a master cannot take it for granted that even his best boy will pass. No fence imposed between boys in the upper forms of public schools and universities should be of this nature. Moreover, many boys in every way likely to make excellent medical men leave school before their future career has been determined. If later on they decide to take up medicine they find that, so far as the University of London is concerned, the door is closed to them by the matriculation examination.

We hold no brief for the London medical schools, but should regard it as a real calamity if London ceased to be one of our chief training grounds alike for consultants and general practitioners. London possesses the raw material for one of the greatest centres of medical education in the world. The public has benefited from the existence of flourishing medical schools in London not only by the opportunities it affords for practical clinical teaching, but to an extent which it in nowise realizes by the presence in its midst of numbers of young medical men engaged in teaching in the schools, and of consultants who are constantly having their minds revivified and their methods kept up to the mark by daily exposure to the criticism of eager, well-informed young students. This advantage London will lose if the tide in the number of entries continues to ebb.

The reference to the Royal Commission appointed last February, with Mr. Haldane as chairman, is very wide. The Commission is empowered to recommend any changes in the organization of the university which may appear to it to be desirable, and in making its recommendations it is specifically instructed to have regard to the provision of facilities for the best education—general, professional, and technical—for persons of either sex above secondary school age. It seems, therefore, abundantly clear that it will be competent for the Commission to receive evidence from the London medical schools as to the means by which the medical degrees may be made more accessible to London students, and it is most satisfactory to find that the dean of one of the schools at least is impressed with the necessity of bringing to notice the defects of the matriculation examination, which are, as he says, well known to all. The defenders of the present system are accustomed to point to the great number of candidates who seek to pass the matriculation examination; but it is well known that a very large proportion of these persons merely require for some reason or other a certificate of general education; they desire to pass the matriculation examination for this reason, but have no intention of proceeding to a degree. Of such candidates many intend to enter the scholastic profession, and it is, of course, very desirable that the university should encourage their ambitions. We submit, however, that the proper way to afford this encouragement is not by imposing upon all would-be undergraduates a matriculation examination which is far more a test of mere knowledge and memory than of the ability to learn, but that a better method of meeting their needs would be to institute a special examination which might include some elementary test in pedagogics.

Before the Royal Commission was appointed the Royal Colleges in London, on the recommendation of a special joint committee, resolved to approach the university with the view of coming to an arrangement under which the university, while retaining all its existing rights as to the granting of degrees, should consent to exercise them as regards pass degrees conjointly with the Royal Colleges so far as students who have spent not less than four years in study at London medical schools and hospitals are concerned. Among the advantages claimed for this plan were that the average London student would be able to obtain the degree of Doctor of Medicine, that medical education in London would become more systematized, that there would be a reduction in the number of examinations, and that the expenditure on them would be diminished.[1] Unfortunately, as we think, this report made no reference to the matriculation examination; it suggested that the colleges should be associated with the university only in conducting the preliminary scientific, intermediate, and final examinations for the pass degrees in medicine and surgery. The Royal Colleges will no doubt place their views before the Royal Commission, but it is essential also that the obstructive effect of the matriculation examination should be brought to its notice, and it is to be hoped that the statement of the Dean of the Charing Cross Medical School means that the representatives of the medical schools will do this. If the matriculation is not modified, there can be little hope that the expectation expressed by the Royal Colleges that by the plan they propose the average London student will be able to obtain the degree of M.D. will be fulfilled.

Finally, it may be pointed out that, though the university is at present hitting by its matriculation examination chiefly the medical schools, there is reason to anticipate that before long it will be found to be hitting the whole of its faculties and constituent colleges and itself. Until within recent years, it enjoyed a sort of monopoly with regard to young men and young women who could not afford to go to Oxford or Cambridge, but desired some university recognition, whether for their own satisfaction or as evidence to possible employers that they had attained a certain standard in the subjects of general education. During the last few years half a dozen universities have arisen in the provinces, and those who want to get university degrees or certificates of one sort or another will presently come to the conclusion that it is preferable to get them from Liverpool, Manchester, Leeds, Birmingham, Sheffield, Bristol, or Wales, rather than from London, if dealing with London means working for an examination badly conceived and worse conducted.

[1] BRITISH MEDICAL JOURNAL, vol. i, 1909, p. 607.

FASTING PRISONERS AND COMPULSORY FEEDING.

WE publish elsewhere three letters relating to an article on this subject which appeared in the JOURNAL of October 2nd. A discussion on the political aspects of the question which is agitating Mr. Keir Hardie and the supporters of militant suffragism would be out of place here, but a few points may be noted. Mr. Mansell Moullin says the Government is acting vindictively because it feels that it has made a mistake. It is equally open to an advocate of the other side to say that the outcry against compulsory feeding is caused by anger at the failure of tactics which it was hoped would embarrass the Government. Dr. Edkins accuses us of bias; this charge we simply deny. It is not bias, but a sense of justice, that leads us to wish that persons who throw slates at policemen and stones at railway carriages should not be allowed to escape the penalty attaching to such conduct because they happen to be women. Dr. Edkins admits that the women who refused food should be punished; but he does not tell us what form of punishment should be used. Does he think that the "good whipping" which Queen Victoria said should be inflicted on a great lady of whom she disapproved would meet the case? In default of that, we do not see what can be done. The prison authorities are responsible for the well-being of those committed to their charge, and they would be guilty of criminal negligence if they allowed a prisoner to commit suicide or deliberately injure his health. If women think it necessary to break the peace by way of vindicating what they believe to be their political rights, they must be prepared to take the consequences. While saying this we wish to protest in the strongest manner against the endeavour that has been made by the Home Office to shift the responsibility on to the shoulders of the medical officers. Mr. Masterman stated in the House that the Home Secretary had been advised on good medical authority that no serious injury was to be apprehended in any case of the kind, even if the treatment had to be prolonged for some considerable time. It may, therefore, be taken as certain that compulsory feeding must have been expressly sanctioned by the Home Office before it was carried out, and the Home Secretary has shown what we can only call contemptible pusillanimity in seeking to shelter himself behind his own official subordinates. In the great struggle that took place about Mr. O'Brien's breeches, Mr. Balfour, to use a historic phrase, "faced the music." We commend his example to Mr. Gladstone. Compulsory feeding in the circumstances must be as unpleasant to the medical officer as to the prisoner, and if the whole responsibility is to be thrown on him, he will find himself face to face with a divided duty—that which he owes to his patient and that which he owes to his official superiors. The Home Office has no right to place him in this cruel dilemma.

THE RIGHTS OF MEDICAL WITNESSES.

ACCORDING to a report in the *Daily Telegraph* of September 30th, a medical practitioner, subpoenaed to give evidence at the City of London Court, who before being sworn, had objected that his lawful fees had not been paid, was told by Judge Rentoul, K.C., that "although not bound to come under the circumstances, being there he must give his testimony." The witness said he was unaware that this was the law, and it must be confessed that this *ignorantia legis* is common to the whole medical profession. As it is a legal aphorism that "*Ignorantia legis neminem excusat*," it would be well that this dictum of Judge Rentoul should be examined, and that it should be made clear whether this latest judicial decision is in accordance with the law of the land. In Glen's manual of the *Laws Affecting Medical Men*, p. 288, we find in reference to the duties of the medical witness: "His reasonable expenses should be tendered to him when served with the subpoena, and even though he actually appear, he cannot be attached for declining to give evidence, unless these charges be paid or tendered, but the objection that his expenses have not been paid must be raised before he has been sworn." Again, in the *Law Relating to Medical Practitioners*, by Craven and Coppock, it is laid down on the same subject, p. 229: "The witness has also another remedy, to refuse to give evidence till compensated for his loss of time, but such objection must be taken before the oath is administered." If the dictum of Judge Rentoul is sound law, the inaccuracy of the above-quoted authorities is sufficiently obvious, and it would seem that the medical witness has no protection in the county court, but may be improperly brought there and then compelled to give his evidence. It may be that county court practice in this respect substantially differs from that recognized in the other tribunals of the country. The matter, however, is sufficiently important to require further elucidation, and in the meanwhile we respectfully dissent from Judge Rentoul's interpretation of the law on this subject.

COURT-MARTIAL ON A FLEET SURGEON.

IN the *London Gazette* of October 1st there appears the following announcement: "In accordance with Her late Majesty's Order in Council, dated April 1st, 1881, Fleet Surgeon Charles Geekie Matthew, M.B., has been placed on the Retired List at his own request. Dated September 30th, 1909." Fleet Surgeon Matthew's retirement is doubtless the outcome of the court-martial, a report of which appeared in the JOURNAL of September 25th, p. 921. He was charged with behaving with contempt towards Commander Richard H. Walters, his superior officer, and with wilfully disobeying his orders. The charges were trivial in themselves, and the whole case against Fleet Surgeon Matthew was of the flimsiest description. The evidence of the commander was not supported by his own subordinate officer, a fact which in itself, in view of the system of terrorism which goes by the name of discipline in the navy, speaks volumes. His own admissions show that he behaved with impertinence to a man his equal in rank and his senior in age. Fleet Surgeon Matthew was acquitted. This, again, is, as Carlyle would have said, "significant of much." To the average man it must, we think, appear that the natural sequel would have been the retirement of the executive officer whose behaviour on this occasion, as shown by his own evidence, would seem to the civilian mind to raise the question whether he is fit for independent command. Fleet Surgeon Matthew, having no chance of redress, has shown his sense of the treatment of which he was the victim in the only way possible to him. The net result of the incident is that the Navy has lost a first-rate officer. However much we may deplore his decision, we cannot affect to be in any way surprised at it. We have published one or two letters, the general tenor of which may be summed up as an expression of belief on the part of the writers that, as far as the Medical Service is concerned, all is for the best in the best of all possible navies. Each man, of course, must speak from his own experience, but the information in our possession shows convincingly that the writers referred to cannot be accepted as the spokesmen of naval medical

officers in general. From that information we get the impression that the medical service of the navy in general is not by any means satisfied with the existing state of things. A number of senior medical officers have expressed their approval of the comments which we made on the court-martial in the JOURNAL of September 25th. Medical officers cannot, without detriment to the interest of the service, be left to the caprice of young men inflated with a sense of their own authority. It is not a mere question of the dignity of doctors; the efficiency of the service is involved. We therefore once more commend the incident and the state of things of which it is the result to the attention of the First Lord of the Admiralty. The retirement of Fleet Surgeon Matthew leads by natural association to another matter. Some considerable time ago it was publicly announced that a Commission had been appointed to inquire into the alleged grievances of the Naval Medical Service, and as to why it should be so unpopular as compared with the Army Medical Service and the Indian Medical Service. There is reason to believe that the work of the Commission has been completed and that their recommendations have been sent to the Board of Admiralty. In view of the announcement that the next examination of candidates for fifteen commissions in the Royal Navy Medical Service will take place on the 1st and following days of November, would it not be wise for all those intending to compete to wait until the result of the Commission and the intentions of the Admiralty have been announced? We have shown that there is reason to believe that all is not well with the Royal Navy Medical Service; it remains to be seen whether the Admiralty is disposed to remove the legitimate grievances which exist.

THE CULTIVATION OF MEDICINAL PLANTS.

THE Hanbury medal of the Pharmaceutical Society, which is awarded biennially, went this year to Professor Alexander Tschirch, rector of the University of Bern, who has conducted some excellent research work on the natural history and chemistry of drugs. He came over to receive his honour from the hands of the president of the society, Mr. J. F. Harrington, and delivered an address at the opening of the fifty-eighth session of the Pharmaceutical Society's School of Pharmacy on September 29th. In the course of his address he remarked that the much ridiculed and derided pharmacists of old times, who extolled the various organs of the animal body and their secretions as remedies seemed to have been in a measure justified in their beliefs by the modern science of organ-therapy. Medicinal plants were the most ancient remedies of mankind, and the experience of thousands of years vouched for their utility. In the misty days of Babylon, in Galen's time, and even until quite recent centuries, nothing whatever was known of the manifold nature of the constituents of drugs. Each drug was regarded as an entity by itself, and as possessed of but a single source of power. Nowadays these matters were better understood, and insight was being gained into the life-process of animal and vegetable cells. A time, therefore, was approaching when it should be possible to solve such problems as that of increasing the amount of some desirable constituent naturally present in a plant, and diminishing constituents which were deemed to be of an undesirable kind. Something of this sort had already been attained; experiments with cinchona trees and with opium plants had proved the feasibility of growing cinchona bark rich in quinine, but containing only small quantities of the other alkaloids, and opium poor in narcotine. It had, indeed, been shown to be possible to increase above the normal the valuable constituents of almost all cultivated plants. It is in this direction that much of Professor Tschirch's own work has lain; for years he has combated the opinion that cultivation necessarily impairs the value of medicinal plants and that from the point of view of materia medica and pharmacology wild specimens are without exception preferable. To a great extent he has succeeded in showing that it is only unsuitable cultivation which produces bad results, and that by varying the conditions of cultivation, by careful selection of suitable strains, by crossing, grafting, and other means, brilliant results can be obtained. On the same afternoon a new chemical laboratory was opened at the school, and the prizes for the year were distributed. The Pereira medal, the blue ribbon among students of pharmacy, was handed to Mr. Sydney Smith. Last year it was carried off by a lady, and this year again two ladies—Miss G. H. Wren and Miss G. M. Neve—were among the six principal prize-winners, carrying off between them the Redwood scholarship, the Burroughs scholarship, and the Pharmaceutical Society's medal.

SIMULATED WEAKMINDEDNESS.

THE last two numbers of the valuable clinical journal for mental and nervous diseases,[1] edited by Professor Sommer, of Giessen, contain a contribution by Dr. T. Becker, of the Prussian Army Medical Corps, upon the "Simulation of Weakmindedness." The paper is based upon two patients sent to the clinic for examination as to their mental state from the law courts, where they had been accused of perjury. In both cases careful systematic examination of the mental status was made by simple interrogation, by word tests and comparisons of reaction times, by Ranschburg's word-pairing method, Ebbinghaus's "completion" method, etc., and the results are set forth by the author with a wearisome infinity of detail. In both cases the simulation of weakness of intellectual capacity and mental grasp was apparent, from the contrast between the subjects' behaviour and conversation when with other patients and the attendants only, and when undergoing examination, and also from their manner of response during repeated tests. Many of the tests of power of observation and memory and of reasoning capacity have been so elaborated that purposely faulty answers can almost certainly be detected. The manner of their detection in the cases under review is interestingly worked out by Dr. Becker. As Dr. Becker points out, however, the question of simulation may be regarded from three points of view: (1) The appearance of simulation may be evoked by weakmindedness; (2) weakmindedness may be purposely and entirely assumed; and (3) simulation may be superadded to existing weakmindedness. There can be no question that the great majority of cases belong to this last category, just as the simulation of pronounced insanity is so commonly found in defective individuals that it is in itself of symptomatic significance. Of the two cases discussed at such length by Dr. Becker, the first was diagnosed as belonging to the third category, and the subject was therefore recommended to the court for the application of the exonerative clauses with regard to cases of doubtful responsibility; whilst the second was found to be a pure malingerer and handed over to the court for punishment. Detailed mention

[1] *Klinik für Psychische und Nervöse Krankheiten.* Bd. iv, Hft. 1 and 2. Halle a. S.: Carl Marhold. 1909. (Sup. roy. 8vo, pp. 194. M. 10.)

of the second case is not needed here, but with regard to the first, it is doubtful whether the evidences of feeble-mindedness adduced would be considered sufficient in this country. These were as follows: Senile arterio-sclerosis; finger tremor of anomalous type, but giving curves (presumably with Sommer's apparatus), recognized as pathological; indifference to surroundings and loss of interest in family relations; confabulation; prolixity and diffuseness in conversation; emotional anomalies (cheerfulness out of harmony with his position); undoubted lengthening of reaction-times; blunted sensory perception, defective apperception and poor reproduction, and failures of memory for recent events.

INFECTIOUS RISKS AT HEALTH RESORTS.

Although not so common an occurrence as formerly, it occasionally happens that one or more children in a family returning from a visit to the seaside are stricken down with an infectious disease caught while away from home. It is to be feared that in many instances the initial infection has been caused by the carelessness, wilful or otherwise, of some person in charge of a convalescent patient. Such carelessness when detected ought not to go unpunished, and we rejoice to find that the Tenby justices on a recent occasion viewed this carelessness in a very serious light. A little boy was certified to have been attacked with scarlet fever on August 18th, and the mother and hospital nurse who was in attendance were told by the medical attendant that they must not permit him to go into the public streets until a certain date. In spite of this instruction the child was taken by his mother on to the sands on September 18th, 19th, and 20th. Upon these facts coming to the knowledge of the medical officer of health (Dr. Douglas A. Reid), he wrote to the child's mother pointing out the danger of such a proceeding and her liability to a penalty, and asking her to undertake to keep the boy in the house for another week. This very reasonable request not being acquiesced in, Dr. Reid very properly proceeded under Section 126 of the Public Health Act, 1875, before the borough justices. The mother, who was represented in the police court by a solicitor, set up the defence that in her opinion the child had not had scarlet fever at all, although no medical evidence in support of her view was forthcoming. The chairman of the magistrates in inflicting a fine of £10 with costs amounting to £3 13s., said that the Bench looked upon the case as a very bad one. The children of a poor man living in a small house were taken to a fever hospital, and those persons who were fortunate enough to live in larger houses could keep their patients at home, but in doing so they were practically on their honour to carry out every possible means of preventing the spread of the disease.

DRUG CURES FOR DRUG HABITS.

From time to time the correspondents in the United States of the *Daily Telegraph* make incursions into the field of therapeutics, not always with the happiest results. The latest example is contained in a telegram from New York, published on October 7th, stating that Dr. Alexander Lambert, physician to the Bellevue Hospital, declares that cravings for cocaine, morphine, or alcohol can be obliterated within three, or in extreme cases within five, days. The ingredients of what is called Dr. Lambert's specific are stated to be belladonna, xanthoxylum, and hyoscyamus. Belladonna is, of course, well known as a remedy believed by many to have a distinct influence in assisting an alcoholic to break the habit; atropine has, in fact, been used for the purpose a good deal in orthodox medicine, and either it or some other alkaloid of the belladonna series is an ingredient of many of the secret medicines so largely advertised. Neither is there any particular novelty in the use of hyoscyamus. There remains, therefore, only xanthoxylum or prickly ash, an American shrub, the bark and berries of which contain certain active principles which, when swallowed, produce a sense of heat in the stomach, more or less general arterial excitement, and a tendency to diaphoresis. The drug has been used as a remedy in chronic rheumatism, and the bark has been chewed as a cure for toothache. A fluid extract is official in the *United States Pharmacopoeia*, and it would be strange if the addition of this drug to a well-known mixture should produce the striking results said to be claimed by Dr. Lambert. The correspondent benevolently adds: "America gives every new school of healing or "religion a fair chance, and to-day physical culture, "osteopathy, mental healing, and other systems of "drugless treatment are just as popular on this side "of the Atlantic with large, educated sections of the "community as allopaths and homoeopaths in "Europe." It is a little odd that a paper commanding the sources of information at the disposal of our contemporary should publish such banal material.

DEMONSTRATIONS AT THE ROYAL COLLEGE OF SURGEONS.

The demonstrations given in connexion with the Museum of the Royal College of Surgeons last year proved so successful that the council of the college has arranged two courses to be given during the present winter by the Conservator, Professor Keith, and the Pathological Curator, Professor Shattock. The first demonstration of the present course will be given by Professor Keith in the theatre of the college on Friday, October 15th, at 5 p.m., on specimens illustrating the various forms of acquired and congenital occlusions and strictures of the alimentary canal. The date and subject of the other demonstrations will be found in our "Diary for the Week." The aim of these demonstrations is to give senior students and medical practitioners an opportunity of studying and appreciating the great wealth of pathological and physiological preparations in the museum of the Royal College of Surgeons of England.

OPENING OF QUEEN ALEXANDRA SANATORIUM, DAVOS.

Our national sanatorium in the Alps, for the successful completion of which the devoted labour of the local secretary, Mr. H. C. Wrinch, has mainly to be thanked, will be open for the reception of patients at the end of the present month, although the inaugural ceremony is to be postponed until the winter season. On the visiting staff are the well-known names of Dr. W. R. Huggard, H.B.M. Consul in Davos, Dr. Arnold F. Bill, and Dr. Florian Buol. The resident staff consists of Dr. Joseph W. Noble, the Medical Superintendent, who has had considerable experience in Davos, and Dr. James Fairley, Junior Resident Medical Officer. The Matron, Mrs. Teesdale, who for some years past has had the entire management of the Davos Invalids Home, will be assisted by a small staff of English nurses with home sanatorium training. Although each patient is to occupy a separate room, the inclusive charge is only 38s. a week. Only early or convalescent cases are eligible. A large number of applications have already been received. Those desiring admission can obtain forms from the local secretary in Davos, Mr. H. C. Wrinch, or from the honorary secretaries in London, Dr. William Ewart, 31, Upper Brook Street, W., and Mr. D. A. F. Vesey, 3, Camp View, Wimbledon Common, S.W.

Medical Notes in Parliament.

Feeding Suffragettes in Prison.—Last week Mr. Snowden asked the Home Secretary if he had any information as to the state of health of the women suffragettes in prison, and whether they were still being fed by force. Mr. Gladstone said he was glad to say that the steady improvement in the health of all the suffragettes continued. He was not absolutely able to answer the question as to whether they were still fed by force. "Force," however, was hardly the right word, because, although, as he understood, three of them were being fed by the medical officers, they were, in a sense, fed without any resistance on the part of the prisoners. He might say that in no case had the stomach pump been used. There was no proof whatever that the women were handcuffed during the time food was administered. Asked by Mr. Arthur Lynch why he so ostensibly put forward the medical aspect of the case, Mr. Gladstone said he put forward the medical officers because it was a medical question, and any responsibility rested upon the medical officers. Mr. Arthur Lynch asked was it usual for medical officers to act on the instructions of the right hon. gentleman or on their own initiative in such cases. Mr. Gladstone retorted: What authority had the hon. member for saying he had given instructions to the medical officers? He might tell him it was the duty of the medical officers to attend to the health of prisoners in their charge, and that was precisely what the medical officers had been doing here. Later, in written answers to questions by Mr. Keir Hardie, Mr. Gladstone said that he had received several reports from the medical officer. From the report received that (Friday) morning it appeared that seven prisoners still refused to feed themselves, but only one of the seven now required to be fed by tube. The others accepted food when administered to them from a feeding-cup. As regards the method adopted to administer food to the women suffrage prisoners, Mr. Gladstone replied that he had received several reports from the medical officer. He found that Blandford's oesophageal tube had not been used; in those cases where a tube was required the ordinary soft rubber feeding tube used in hospitals was employed. No screw gag was used, and the prisoners were not strapped down. The number of attendants present varied from one to five. He did not think it was desirable that he should mention the names of the prisoners. He was satisfied that everything had been done with the utmost gentleness possible in the circumstances; and the prisoners themselves had borne witness to the kindness of the officers. Questioned on Monday by Mr. Keir Hardie, Mr. Masterman said that the medical officer reported that no injury to the throat, teeth, or other parts of the body had been sustained by any of the prisoners who had undergone the special hospital treatment mentioned in the question. The Home Secretary was advised on good medical authority that no serious injury was to be apprehended in any case of this kind, even if the treatment had to be prolonged for some considerable time. He had not seen any reports from eminent medical authorities to the effect that this treatment always produced permanent injuries and very often fatal results if continued. Further questioned as to the frequency of the practice of administering food by force at Winson Green Gaol, Mr. Masterman said that, with the exception of these women prisoners, there had been no cases in Birmingham Prison during the past twelve months in which it had been necessary to feed any prisoner forcibly. This treatment had been administered in some cases by the medical officer, and in other cases by a medical practitioner who had been called in to assist him. The method had been, where practicable, feeding by means of a teaspoon or from a feeding cup, but where necessary the soft rubber tube in use in hospitals had been employed. Mr. Keir Hardie next asked if the representatives of the suffragettes had not petitioned for leave to send their own medical adviser to report on the condition of the prisoners. Mr. Masterman said that applications to the effect indicated had been received, but the Home Secretary had been unable to accede to them. The prisoners mentioned were receiving full and constant attention from the medical officer of the prison, who had been in consultation with the medical authorities of the Home Office at the time when the refusal to take food made the prisoners' state of health serious, and had had the assistance of a medical man in practice in Birmingham, who had been present when food had been administered. According to the latest report received that morning, the health of the prisoners continued to improve. Only one of them still refused to take food without the use of a tube. On Wednesday Mr. Keir Hardie again questioned the Home Secretary as to the opinion of Dr. Forbes Ross that the method of feeding employed was frequently followed by chronic pigmentary colitis, and that asylum patients fed in this way died as a rule of the treatment. Mr. Gladstone said that results such as described could not follow the method of feeding adopted at the Birmingham Gaol. Mr. Keir Hardie then asked if the Prime Minister had received a memorial signed by Sir Victor Horsley and 116 other medical practitioners pointing out that the compulsory feeding of women was attended with the gravest risks, that unforeseen accidents were liable to occur, and that health might be seriously injured. Mr. Herbert Gladstone said the memorial contained statements which were not borne out by the prison medical officer, whose experience of artificial feeding was necessarily much wider than that of the memorialists. He thought it right to refer the memorial to the President of the Royal College of Physicians, and he had that morning received the following letter from Sir Richard Douglas Powell:

> I beg to state that I do not agree with the opinion expressed in the memorial signed by some medical practitioners in protest against the artificial feeding of certain prisoners who refuse to take food in the formal manner. The statements contained in the memorial are inaccurate and greatly exaggerated. The method of artificial feeding is largely employed amongst those who, from mental aberration, refuse to take nourishment otherwise, and also with those who are physically unable to take it. It would be an exaggeration to say that the method of artificial feeding is wholly free from the possibilities of accident with those who forcibly resist, but no such cases have come to my knowledge. Nor can it be said that the method is as convenient or as free from possible digestive ailments or discomforts as a reasonable diet taken naturally would be. But it may be remarked that patients have been so fed successfully for years. It is the only measure that can be adopted to maintain the health and preserve the lives of persons who persistently refuse food. I am assuming in my reply that the feeding of these patients is entirely carried out by skilled nursing attendants under careful medical observation and control.

In reply to further questions, the Home Secretary said that he understood that the feeding was carried out by a medical officer, and an outside medical officer had been called in to assist; he was satisfied that all had been done in the most proper and humane manner.

The Housing and Town Planning Bill passed the report stage in the Lords on Monday. The attitude of the opposition was much less vigorous than in the Committee, but several amendments were made. Lord Salisbury gave notice of an amendment on the third reading to include Crown lands within the purview of the bill. Lord Beauchamp made a series of amendments to Clause 10 which required the Local Government Board before enforcing their powers on complaint to take steps to inquire locally into the whole question of the need of housing, etc. The Duke of Marlborough then proposed to cut out all reference to the working classes in Clause 14, which, he thought, confused the statement of the obligation of the landlord and the definition of the working classes. It was best expressed, he thought, in money rent. After considerable discussion the amendment was agreed to. The question of rent was then raised by Lord St. Aldwyn, but after some figures given by Lord Beauchamp he agreed to withdraw his amendment and allow the old rent standard of £16 to be restored to the bill instead of £8, which he had carried in committee. After another amendment had been made to the clause, Lord Clinton altered Clause 15 by making

the time of notice of closing on part of landlord twenty-eight instead of twenty-one days. The landlord was also given, on the motion of Lord Kimberley, the option of appeal to the Local Government Board or to the county court on the question of repairs, and a similar option was given in Clause 17 with reference to closing orders. An amendment with regard to underground rooms, moved by the Government, was accepted after it had been slightly amended. Clause 26 was also amended by the Government, and then on Clause 30 Lord Clinton moved several amendments to provide that expenses incurred by a rural district council, under Part III, should be charged on the whole area of the authority unless otherwise declared by the Local Government Board. The amendments were opposed by Lord Beauchamp, and supported by the Duke of Northumberland and Lord Camperdown, and finally passed. Lord Onslow then carried, after a good deal of discussion, a clause requiring the local authority in providing houses under the Act to see that they were occupied by persons of the working classes and British subjects. The Duke of Marlborough next amended Clause 47 so as to provide that the words working classes in the Act should "include mechanics, artisans, labourers, and others working for wages and persons whose income did not exceed an average of 30s. a week." The Town Planning clauses were altered somewhat, and on Clause 59, dealing with compensation, Lord Salisbury amended the subsection by providing that the arbitrator could award the local authority such proportion of their expenditure in adding to the value of property by a scheme, as he might think just, instead of one-half as in the bill. Clause 63 was struck out, and a new clause substituted, and a number of drafting amendments made in the other clauses and schedules. The bill was then reported after Lord Beauchamp had said that the Government could not hold themselves responsible for the changes in procedure introduced.

Death from Erysipelas following Vaccination.—Mr. B. S. Strauss called attention on Monday to the case of a child who died after vaccination from erysipelas, and asked what precautions were taken to ensure the absence of erysipelas organisms from the Local Government Board lymph. Mr. Burns answered that he was aware of the verdict of the coroner's jury with regard to the death of this child. His attention was called to the case, and he caused inquiry to be made with regard to it. It appeared that an interval of at least seventeen days had elapsed between the vaccination and the commencement of the erysipelas, and, in these circumstences, it seemed clear that the erysipelas was not introduced with the vaccination, as otherwise it would have made its appearance at a much earlier date. The child was not vaccinated by a public vaccinator, or with lymph supplied by the Local Government Board; but, as regards the lymph which was so supplied and which was that used by almost all public vaccinators, he might state that very complete precautions were taken for the exclusion from it of erysipelas micro-organisms. These precautions included the use of only healthy calves, the careful watching of the calves while the lymph was being developed, the collection of lymph from those calves only whose vaccination ran a perfectly normal course, and the subsequent slaughter and veterinary examination of all the calves from which lymph had been obtained. The lymph was prepared for distribution in sterile apparatus, and was mixed with glycerine which had been proved to destroy the organisms of erysipelas. Moreover, the absence of such organisms was repeatedly tested before the lymph was distributed.

Vaccination (Origin of Lymph).—Mr. Pointer asked the Secretary of State for the Home Department whether he would cause inquiry to be made into the practice of vaccination to ascertain whether the lymph now used had its origin in or was in any way connected with variolous matter; and, whether, should his inquiries go to show that variolous matter was the origin of the present-day lymph, he would institute proceedings under Section 32 of the Vaccination Act of 1867 against persons using such lymph. Mr. Burns said he had been asked to reply to this question. Section 32 of the Vaccination Act, 1867, rendered it an offence to produce, or attempt to produce, small-pox in any person by inoculation with variolous matter or by any other means. Most of the lymph used in cases of primary vaacination was that supplied from the Government lymph establishment. That lymph did not produce, and was not used to produce, small-pox, and persons using it did not come within the section referred to. There was no reason to doubt that the same observations applied with regard to other lymph in use in this country. There did not appear to be any need for inquiry on the subject.

Old Age Pensions and Medical Relief.—Mr. Cecil Harmsworth asked whether an old age pensioner was entitled to claim from the guardians indoor or outdoor medical relief; and, if so, whether the acceptance of such relief disqualified the pensioner for the receipt of his pension. Mr. Burns said that if an old age pensioner applied to the guardians for medical relief it was their duty to consider whether any such relief should be given him, and, if so, in what form. The receipt of any medical or surgical assistance supplied by or on the recommendation of a medical officer would not in itself disqualify the pensioner for the receipt of his pension.

Lavatory Accommodation for Workmen.—Mr. Wardle asked the Secretary of State for the Home Department whether his attention had been called to the practice extensively carried on in Germany and America whereby wardrobe and lavatory accommodation was provided for workmen at the place of their employment; and whether, in view of the benefit in public health and cleanliness to be derived from such a practice, he would consider the advisability of securing the extension of the practice in this country. Mr. Gladstone said that his attention had been called to this matter, and, in connexion with mines, it had received the consideration of the Royal Commission on Mines, in their recommendations on the subject in Part XVIII of their recently-issued Report. In the case of a number of trades where the conditions of work make the provision of lavatory and cloak-room accommodation especially important for the health of the workpeople, the employer was now required under the Factory Acts to provide it; and in many other cases such accommodation was supplied voluntarily by employers. The Report of the Chief Inspector of Factories for 1908 notes, he was glad to say, a growing tendency to provide improved facilities for washing in many districts, and the question was receiving the constant attention of his department.

Housing in West Ham.—Last week Mr. Tyson Wilson asked the President of the Local Government Board if he could state the result of the inquiries into the sanitary condition of the houses at West Ham in which five children had lived and whose deaths were, according to the verdict of the coroner's jury, due to consumption. Mr. Burns replied that he understood that the houses in question had all been inspected during the last eighteen months, but a further inspection had now been made. Certain defects had been found to exist in some of the houses, and notices had been served upon the owners, requiring them to remedy these defects. The notice had been complied with in every instance except one. In that case part of the work had been done, and legal proceedings against the owner had been threatened if it was not completed at an early date.

A Short Holiday.—On Wednesday, in stating the probable course of business, the Chancellor of the Exchequer said that the Government proposed to take the Development Bill on the Report stage on Thursday and to finish that stage and get the third reading on Friday, or if necessary on Saturday. As soon as the Development Bill was read a third time, the House would adjourn till Monday, October 18th. The London Elections Bill would then be taken, and the Report stage of the Finance Bill on Tuesday, 19th inst.

Scotland.

[FROM OUR SPECIAL CORRESPONDENTS.]

ROYAL COLLEGE OF SURGEONS, EDINBURGH.

IT will interest all old students and members of the College of Surgeons, Edinburgh, to learn that recently the Surgeons' Hall has been undergoing important structural alterations which will add largely to its accommodation and usefulness. Those who are interested in the college are proud of the fact that it has a historical record as a body of over four centuries, for the fourth centenary was celebrated in 1905, an event which is to many a very pleasant memory. The college connects the present age with a period when the profession of surgeon was still linked with that of the barber. As in London, the practitioners described themselves as barber-surgeons, and it was at the instance of the barber-surgeons of the period that the college was founded. The charter of the college dates from 1505; the meetings of the college were held for some years in the house of the Deacon or President for the time being; and the first special meeting place of which there is a record was established in premises in Dickson's Close, the first close east of Niddry Wynd, at a rental of £40 a year. Then, in 1669, what was called a "convening house" was built on the lands of Curriehill, within the south-east angle of the city wall, overlooking the "Pleasance." The erection of the Surgeons' Hall in that locality was the origin of Surgeons' Square, which became a well-known medical centre, and was associated with the story of Dr. Knox and his dealings with Burke and Hare. In 1697, on the same site, an improved and more pretentious building was erected. These buildings were really the birthplace of the great Edinburgh Medical School. In 1832 the "Chirurgeons' Hall" of 1697 was relinquished, and its place was taken by the imposing structure now familiar in Nicholson Street. The college no longer exists as a teaching body, although it owns adjoining class-rooms, which are rented to medical teachers. The chief function of the college is that of an examining body. Surgeons' Hall provides accommodation for that purpose, but it also houses the museum, acknowledged to be one of the finest of its kind in the kingdom, free to all medical students, and also to the general public within certain limits. It is the meeting place of the members of the college, and scientific lectures are given there from time to time. The college possesses also an extensive library. The alterations, which have been in progress for some time, affect all these departments. They give additional accommodation without interference with the coherence and proportions of the interior. The council room, which occupied the greater part of the ground story, has been vacated as such, divided into two, one part being added to the library, and the other part forming a reception room. The western portion of the old museum, devoted to the Barclay collection, has been converted into the new council room. To provide for the Barclay collection certain tenement houses have been converted into additional museum spaces. The design of the original museum has been followed in the arrangement of the galleries, but glass shelves have been introduced so as to give good light. The flooring of the galleries has been formed of glass with the same object.

Altogether, the college authorities are to be congratulated on the manner in which these alterations have been carried out; they will undoubtedly be approved by all Fellows and Members.

THE CHAIR OF ANATOMY, EDINBURGH.

Professor Arthur Robinson, the successor of the late Professor Cunningham in the Chair of Anatomy in Edinburgh University, was introduced to the students of the Faculty of Medicine on October 1st, in the McEwan Hall. Sir Wm. Turner, who presided, was accompanied to the platform by the members of the professoriate. After being introduced by the Principal, Professor Robinson delivered an address entitled, "A Glance at Anatomy from 1709 to 1909." He sketched the history of the chair down to the occupancy of Sir William Turner, to whom he paid a high tribute, stating that his influence as a teacher of anatomy might be gauged to some extent by the fact that his pupils held, or had held the Chair of Anatomy in Edinburgh, Glasgow, St. Andrews, Belfast, Newcastle, Manchester, Liverpool, Cardiff, Oxford, Dublin, Melbourne, Sydney, Montreal, and Toronto. He knew of no other such record. After paying a fitting tribute to his immediate predecessor in the chair, Professor Cunningham, he said that in the past 100 years all branches of anatomy had made more progress than in any previous time. Improved methods and improved instruments had enabled an army of observers and experimenters to obtain results never dreamt of in the earlier days, and the occupants of the Chair of Anatomy had ever kept themselves in touch with the more important part of the rapidly growing subjects, and had transmitted to their students a knowledge of all the chief facts. There was little doubt that if the anatomists of the latter part of the eighteenth and early part of the nineteenth centuries could return to-day, they would be amazed by the amount of knowledge which had been accumulated since their time. Nature, as revealed by anatomy, showed plainly that she had no beliefs in any general equality. She produced her children in myriads, cast them into the sea of circumstances, approved and helped only those who could swim, and rewarded them according to their performance. The riddle of life was not yet solved, but members had sought and would seek, and in the search someone would find in the future, as some had found in the past, knowledge useful to mankind.

THE CHAIR OF SURGERY IN EDINBURGH.

Mr. Alexis Thomson, who has succeeded Mr. John Chiene in the Chair of Surgery in the University of Edinburgh, was formally introduced in the McEwan Hall on October 4th. Principal Sir William Turner presided, and there was a large attendance of the Senatus Academicus on the rostrum. Professor Chiene and the new Dean of the Faculty of Medicine were present. After giving a eulogy of Professor Chiene, the Principal said they were there along with Professor Chiene and the students to give a welcome to his successor. Professor Alexis Thomson had worked laboriously in that school for many years. He was recognized as an accomplished surgeon, a sound pathologist, and an able teacher.

Professor Thomson said he was deeply conscious of the responsibilities which rested on his shoulders in undertaking the duties of the Chair of Surgery in that great university. He was emboldened, however, by the encouragement which he had always received from his esteemed teacher, the well-beloved and venerable Principal, Sir William Turner. He relied upon the loyalty of his colleagues in the Senatus—some of them his former teachers, a sadly diminishing number; some of them his former fellow-students, and among these he was proud to include the new Dean of the Faculty of Medicine.

The Chair of Surgery, he said, was of comparatively recent foundation. From 1777 until 1831 it was held conjointly with that of Anatomy. The first Professor of Surgery, John William Turner, only held the chair for five years. He was succeeded in 1836 by Sir Charles Bell, who was in turn succeeded by James Miller. Then followed James Spence (1864–1882), and his successor was John Chiene. Since Mr. Chiene was appointed Demonstrator of Anatomy under Goodsir, forty-three years ago, he had been a living force and a tower of strength in that medical school and university. In the Edinburgh of the Seventies Chiene was universally recognized as the most ardent and most faithful disciple of Lister. There had always been good teachers of surgery and brilliant operators in that medical school, but at the critical period when the science of surgery was being revolutionized it was a fortunate thing for Edinburgh to have first as Extra-mural Lecturer and then as Professor in the university a man with the moral force,

the strength of character, enthusiasm, and determination of John Chiene. The surgery of the present, like every other department of human knowledge, was the product of evolution, and had culled from every source that which would contribute to its advancement. A new era dawned when John Hunter left the Lanarkshire farm in which he had been born and brought up, and joined his elder brother William in London, three years after the rout of the Stuart cause at Culloden. It was Hunter who raised surgery to the dignity of a science. Before his time surgery and physiology were far asunder. Never since Hunter's day had the study and teaching of surgery been dissociated from the sciences of physiology and pathology. Surgical anaesthesia, which they owed to America (the word itself to the gifted "Autocrat of the Breakfast Table"), was of all gifts of science to mankind the happiest and the most easily appreciated. The name of their countryman, James Y. Simpson, would always be associated with the birth of anaesthesia, for he not only discovered the anaesthetic uses of chloroform, but he rendered memorable service in advancing its cause. It was Joseph Lister who reformed the whole science of surgery, and it was one of the priceless possessions of their *Alma Mater* that it was during his incumbency of the chair of clinical surgery in that university that he perfected the theory and practice upon which the antiseptic principle was founded. Impressive and encouraging as was the remarkable growth, within recent years, of laboratories devoted to medical science, no one who had any knowledge of the vast field to be covered, of the difficulty and complexity of the problems, of the expenditure of money required, and of the returns in increased knowledge and benefits to mankind which had been attained, and which might be expected in increasing measure, could for a moment suppose that the existing opportunities, considerable as they were, were adequate to meet the present and the future needs of scientific medicine. It must ever be the aim of those who had the interests of universities and colleges at heart to foster the spirit of research in their students, and especially in their graduates, and ever seek to provide greater facilities to enable such research to be undertaken. They as a profession did not grudge the large expenditure which was required to maintain the naval and military forces of the Empire, nor the providing in old age for those who were unable, or had honestly failed to provide a competence for themselves, but they did desire that in the huge expenditure with which the country was charged that more should be forthcoming than the beggarly pittance at present granted for the advancement of those sciences on which the health and happiness of the people so much depended.

Dermatology in Glasgow.

In accordance with the power recently obtained by the directors in a Provisional Order, the Skin Hospital in Elmbank Street, Glasgow, has been closed and ceases to exist as a separate institution. The outdoor department has been transferred to the Western Infirmary, and will be carried on as a department of the dispensary, while, as hitherto, the two skin wards will be available for the treatment of more serious cases. To carry on the new dermatological work, the directors of the Western Infirmary have appointed Dr. J. Wyllie Nicol physician to the wards, and Dr. J. G. Tomkinson to officiate on the dispensary staff. During the next few months the University of Glasgow will establish the McCall Anderson Memorial Lectureship in Dermatology, for which provision is also made in the provisional order.

During the forty-eight years of its existence the Skin Hospital has done valuable work. Originally started for the gratuitous treatment of the poor suffering from the various forms of skin disease and for the advancement of this department of medical science, under the able guidance of the late Sir Thomas McCall Anderson both these objects were nobly fulfilled. The hospital was founded in 1861, and the first physicians were Dr. A. B. Buchanan and Dr. T. McCall Anderson. Dr. Buchanan died in 1865, but McCall Anderson maintained his connexion with the hospital till his death in 1908. The original premises were at 63, John Street, where they remained till 1878, when the directors purchased a house in Holland Street with an entrance from Elmbank Street. In 1871 the directors made a good bargain with the managers of the Western Infirmary, then in course of erection, whereby, on the sum of £2,000, which they had collected for building purposes, being transferred to the Western Infirmary that institution undertook to provide for the use of the Skin Hospital two wards each of 20 beds. It will thus be seen that the connexion between the Skin Hospital and the Western Infirmary is of old standing, and it was natural that when, on the death of Sir Thomas McCall Anderson, the future of the Skin Hospital came to be discussed, the directors should decide that the main objects of the Skin Hospital would be most advantageously ensured by amalgamating with the Western Infirmary and by founding a lectureship in the university for the promotion of study and research in relation to the pathology and treatment of skin diseases and for giving practical instruction in dermatology. To endow the lectureship the sum of £2,500 is to be paid over to the University Court, and the lectureship is to be named the "McCall Anderson Memorial Lectureship in Dermatology." The Western Infirmary has also agreed to call the two skin wards the "A. B. Buchanan Memorial Ward" and the "McCall Anderson Memorial Ward" respectively.

Ireland.

[From Our Special Correspondents.]

Dispensary Doctors' Salaries.

At a meeting held on September 27th, the Limavady Board of Guardians, despite the appeal of Dr. MacCarthy, Local Government Board Inspector, to their feelings of fairness and justice, to say nothing of generosity, decided to adhere to the scale of salaries previously adopted. This scale, proposed in 1905, provided for an initial salary of £100 a year, proceeding by increments of £5 quinquennially to a maximum of £120. At present four of the five dispensary medical officers are in receipt of £100 a year and the fifth £105, although they have been in the service of the guardians for periods of thirty-seven, thirty-one, thirty, nineteen, and seven years respectively. It was intended by the guardians that the first increase of £5 should only be granted on the completion of *five more years*. It was thus manifest that these gentlemen could never reach the very meagre maximum allowed. Dr. MacCarthy said the scale was absolutely the very lowest in Ireland. Once medical men have been for some time established in such appointments and districts, it is extremely difficult to abandon all their interests and seek new fields; but how any young medical man, with all England, the services, and the colonies before him, can be induced to apply for such posts and be at the beck and call of every red line, perhaps necessitating a journey of fifteen miles, and at a salary which, all told, will scarcely pay his posting, can only be explained as a matter of sentiment or ignorance of the world, not on principles of legitimate business. The landed gentry have left or are leaving the country houses. Such a house, with attendance on visitors, servants, and mostly a large family, used to be worth anything from £20 to £100 a year to the medical attendant, but they are standing empty by the dozen all over Ireland; the farmers are growing wealthier, but from a medical point of view offer no compensation as yet to the unfortunate doctor.

Health of Belfast.

At the monthly meeting of the Belfast City Council on October 1st, the medical officer of health reported that the death-rate from all causes had been 15.7; there had been a few cases of typhus fever, but the disease had been stamped out, and at the beginning, and no others had occurred. At the next meeting

The Port Sanitary officer and the medical officer of health reported the importation of some 140 tons of inedible lard from the United States of America; the word "inedible" had been whitewashed over. The importers agreed to reinstate the words "inedible" or "non-edible," and to account for all such lard they already had or were about to receive. Professor Symmers reported that the water in the Ormeau Avenue Baths, after four weeks' use of the new system of filtration and aëration, contained 27 per cent. less germs than the original fresh water.

THE BELFAST NATURAL HISTORY SOCIETY.

As already announced in the JOURNAL, Sir John Byers has been re-elected President of the Natural History and Philosophical Society for another year. The society has made application to the Commissioners of Charitable Donations and Bequests to be allowed to transfer upon loan to the Belfast Corporation the various collections of the museum, and the Corporation has agreed to take this over, and strike a rate of ½d. in the £ for the care and use of the specimens. This will leave a certain amount of money to be expended on lectures and the like.

England and Wales.

[FROM OUR SPECIAL CORRESPONDENTS.]

MANCHESTER AND DISTRICT.

THE JOHN MORLEY LABORATORIES.

THE new chemical laboratories, which have been built through the munificence of Mr. Andrew Carnegie, who presented £10,000 to the University of Manchester for the purpose, are, by his request, to be known as the John Morley Chemical Laboratories. The new buildings include a large laboratory for organic chemistry, research rooms, private laboratories, and other accommodation for the professors, and a large laboratory for third-year students, with new service and store rooms, and cold-storage and ice-making machinery. In addition to Mr. Carnegie's gift a considerable sum is required for equipment, but towards this some generous benefactions have already been received.

Lord Morley, the Chancellor, who presided at the Convocation of the University on October 4th, after conferring honorary degrees upon the American Ambassador; Sir Robert Stout, Chancellor of the University of New Zealand; Sir Alfred Lyall, and Dr. Otto Wallach, Professor of Chemistry in the University of Göttingen, delivered a short address, in the course of which he said that he would like every one to consider what was the proper answer to the question, What is an educated person? He would himself put first the power to appreciate the nature of evidence, to know when a thing was proved and when it was not. His experience at the India Office had led him frequently to consider how far circumstances limited the application of abstract principles, and how far circumstances transformed principles, excellent in certain respects at certain times and in certain places, into irrelevant catchwords. The American Ambassador, Mr. Whitelaw Reid, in thanking the university for the honour conferred upon him, said that in some of the newer institutions in America there had sprung up an idea, rather extravagantly expressed in the popular saying that the education of a boy who was to make his way in the world should teach him everything about something and something about everything. A university like Manchester might be disposed to be little more insistent upon the last principle.

Sir Henry Roscoe, Emeritus Professor of Chemistry, having declared the new laboratories open, said that the chemical school of Owens College and the University of Manchester had been, and he firmly believed would long remain, the premier school of chemistry in the country. The new laboratories were admirable, and he was glad that Mr. Carnegie, who had endowed libraries throughout the English-speaking world, should now have taken his share in establishing a scientific laboratory, he hoped not for the last time. It was the teacher's personality that made a school, and in that respect the university was on sure and safe ground. In the course of some reminiscences, Sir Henry Roscoe said that it was almost exactly fifty-two years since, at the age of 24, he was appointed professor of chemistry in Owens College; he was the last survivor of the old staff which in 1857 set itself doggedly to overcome the prejudices that then existed. The college was then a very small affair; there were only thirty-five students, of whom fifteen worked in his laboratory. The progress made in the last fifty years read almost like a fairy tale. The university was now conspicuous in all branches of intellectual activity; it was not a mere school of science, though each of its science departments might well challenge comparison with similar institutes in other universities, but a real university with all its faculties fully and adequately represented.

WALES.

SWANSEA SANATORIUM.

THE Swansea Board of Guardians, which has for some time recognized the need of a sanatorium in the Swansea district, has now decided to call the attention of the Swansea Borough Council, as the authority primarily responsible for the health of the town, to the urgent necessity of doing something to prevent the spread of consumption either by providing a sanatorium, or such other means. Mr. Owen, who has taken an active interest in the subject, told the guardians that last week one of the relief committees had thirteen cases of consumption before it. Sir John T. D. Llewelyn, of Penllergaer, recommended that an arrangement should be made with the Alltymynydd Sanatorium, which, he said, had already done excellent service. At present that sanatorium was restricted to the three western counties, but he hoped some arrangement could be made for the erection of châlets to which Swansea patients could be sent. Recently, as a representative of the Glamorgan County Council, he attended a conference in London, at which the feeling was expressed that local authorities might join in a much wider application of powers to get this terrible disease stamped out. He understood that about 50 per cent. of consumptive cases could be cured if sent away in decent time. Some of the members attributed much of the consumption at Swansea to overcrowding.

Correspondence.

THE GASTROSCOPE AND ITS USES.

SIR,—It may safely be assumed that the vast majority of those who read in your issue of September 25th the account of the admirable piece of independent work carried out by Drs. Souttar and Theodore Thompson on gastroscopy, were under the impression that these investigators had struck new ground. Only a select few in all probability were conversant with the pioneer gastroscopic inventions and demonstrations of Mikulicz[1] so far back as 1881, and with the straight gastroscopic devices introduced by Rosenheim[2] in 1895. There must be many, however, who remember reading the lengthy and illustrated article on oesophagoscopy and gastroscopy in the *Lancet* of April 28th, 1900, by Dr. George Kelling,[3] in

[1] V. Mikulicz, J.: (*a*) Ueber Gastroskopie und Oesophagoskopie, *Zentralbl. f. Chir.*, 1881, No. 43, s. 673. (*b*) Ueber Gastroskopie und Oesophagoskopie, *Wien. med. Presse*, 1881, No. 45, s. 1405; No. 46, s. 1437; No. 47, s. 1473; No. 48, s. 1505; No. 49, s. 1547. (*c*) Ueber Gastroskopie und Oesophagoskopie. mit demonstrationen am Lebenden, *Verhandlungen der Deutsch. Gesellsch. f. Chir. XI Kongress, Berlin, 1882*, s. 30. (*d*) Leading article on Oesophagoscopy and Gastroscopy, *Medical Times*, May 5th, 1883.

[2] Rosenheim, Th.: (*a*) Ueber die Besichtigung der Kardia nebst Bemerkungen ueber Gastroskopie, *Deut. med. Woch.*, 1895, No. 45, s. 740. (*b*) Ueber Oesophagoskopie und Gastroskopie, *Deut. med. Woch.*, 1895, No. 43, s. 688. (*c*) Ueber Gastroskopie, mit demonstrationen, *Berl. klin. Woch.*, 1896, No. 13, s. 239, u. s. 275.

[3] Kelling: (*a*) Zur Oesophagoskopie und Gastroskopie, *Archiv f. Verd.-Krank.*, 1896, Bd. ii, s. 321. (*b*) Endoscopy of the Oesophagus and Stomach, *Lancet*, April 28th, 1900.

which a new form of flexible gastroscope was described and figured.

Again, those who attended the Cavendish Lecture at the West London Medico-Chirurgical Society by Professor Mikulicz some three or four years ago had an opportunity of inspecting that surgeon's improved gastroscope. In March, 1907, Chevalier Jackson's illustrated monograph on "Tracheo-bronchoscopy, Oesophagoscopy, and Gastroscopy" was published, and the favourable review which appeared in the British Medical Journal must have been widely read. Jackson's work contains no less than thirty-two coloured pictures of gastric appearances in health and disease, extending from the cardia to the pylorus, and made from views obtained by means of his *straight* rigid gastroscopic tube. Finally, some of those who possess vol. iv, part ii, of Allbutt and Rolleston's *System of Medicine*, published just over a year ago, must have noticed the short section on gastroscopy by Mr. Waggett.

The authors of the latest attempt to construct a serviceable gastroscope appear to belong to the majority who were unacquainted with the achievements of previous workers on the endoscopy of the stomach *per vias naturales*, for they make no reference whatever to the labours of others in this field of research, and the communication opens thus: "It is now nearly three years since we first attempted to solve the problem of gastroscopy." As it is in the highest degree improbable that these able investigators would be guilty of *suppressio veri* and *suggestio falsi*, we may safely conclude that unacquainted with, and unaided by, the efforts of others, they have independently and successfully devised a gastroscope which, whatever its deficiencies (obvious enough to an experienced endoscopist) has enabled them to get a really good view of the interior of the stomach when once the instrument has been introduced *without mishap* into that organ. One cannot but regret that Dr. Souttar did not commence where previous gastroscopists had left off rather than approach the subject *de novo*, without the instruction afforded by the successes and obvious shortcomings of the work of others; for in that case there is, to my mind, little doubt that with his industry and mechanical genius he would have devised in three months a more perfect instrument than this one which represents, as he tells us, three years' work. When Dr. Souttar set out "to solve the problem of gastroscopy," he soon arrived at the conclusion that "beyond the cardiac orifice a straight rod cannot be carried, for it will impinge on the lower dorsal vertebrae where their bodies form the lower portion of the dorsal concavity. The lower end of our instrument was therefore bent forwards at an angle of 30 degrees." Now this was the mistaken assumption made by Mikulicz in 1881; he adopted a tube with a beak bent at the same angle as Dr. Souttar's, and he published a diagram with his gastroscope in position to illustrate this point. This diagram so closely resembles Dr. Souttar's illustration that we may be certain that they each independently borrowed this picture from the same source (probably some well-known anatomical work). Yet at the very time that Dr. Souttar was embarking on his gastroscopic investigations, Jackson's gastroscopic tube had been sufficiently used to show that the pylorus could be reached with a straight tube, despite the assumed obstructing bogey, the body of the tenth dorsal vertebra. In order that this statement should not be merely taken on trust, Jackson reproduces in his monograph an *x*-ray photograph showing the end of his tube in the region of the pylorus in a living subject.

Those in recent years who have attempted to construct a safe and generally practicable gastroscope have fortunately been experienced oesophagoscopists. The statement that the blind passing of an angular gastroscopic tube *without a previous oesophagoscopic inspection* is a proceeding "entirely devoid of danger," as Dr. Souttar ventures to assert, is one which will be challenged by any surgeon who has had any considerable experience in investigating morbid conditions of the gullet by modern methods, and it is a procedure which will be condemned by all who have any cognizance of fatal cases of blind instrumentation of the gullet. The capacity for inflicting serious injury of a flexible oesophageal bougie blindly passed must be comparatively slight compared with that of a rigid *angular* tube. Yet blind bougieing of the oesophagus is recognized by expert oesophagoscopists as dangerous and unjustifiable, and one authority has even described it as criminal. Only to-day a patient suffering from dysphagia refused to allow a well-known laryngologist to endoscope his gullet on the ground that his best friend had been fatally injured by the blind passage of a bougie. Mr. Waggett recently told me that he knew of two cases in which death resulted from blind bougieing, and my colleague, Dr. Spilsbury, informs me that he has made *post-mortem* examinations on two patients who had succumbed to perforations of the gullet caused by bougies. It may be argued that, where disease of the gullet exists, the blind passage of a gastroscopic tube would never be attempted. It is only, however, by oesophagoscopy that we can be quite sure that there is no contraindication to the passage of a gastroscopic tube. Latent lesions often exist without obvious symptoms; dysphagia, for instance, is usually a very late symptom of malignant disease.

The ideal gastroscope should include a straight rigid tube which can be passed through the oesophagus into the stomach *under visual inspection*. Jackson's gastroscope fulfils this condition. If it be proved desirable, as appears likely, to have a distal apparatus inclined at an angle and revolving, then the improved gastroscope should include an inner tube with a joint near its distal end, which could be passed as a straight tube through the outer straight oesophageal portion of the instrument, and afterwards bent at the desired angle at its distal end when well within the stomach. The pharyngeal angle in Dr. Souttar's instrument, and also in Kuttner's, with its reflecting mechanism, is unnecessary, and a disadvantage on account of the varying length of gullets; the oesophageal part of the instrument should pass straight out as in the oesophagoscope, its proximal end being at the angle of the mouth near the molar teeth, with the head extended.

Dr. Thompson speaks of his gastroscopic observations as "direct inspection of the gastric mucous membrane." With two angular reflections by means of mirrors it is obvious that the view is exactly the opposite of direct. Jackson's method *is* direct gastroscopy; the method of Dr. Souttar and Thompson is, of course, indirect.

Although Jackson has devised a practicable and comparatively safe method of gastroscopy, and has removed portions of growths through his instrument for microscopical examination, I believe the procedure of examining the stomach in an empty condition—a necessity in Jackson's instrument—is a distinct drawback compared with the cystoscopic distension method, originally employed by Mikulicz, and now adopted by Dr. Souttar; and I cannot but think that the latter's gastroscope, when modified and rendered as safe as Jackson's, will take its place as the most serviceable one in the immediate future. In its present form its employment, in my opinion, is hardly made justifiable.

Finally, I wish to state quite clearly that my own experience in gastroscopy is of the slightest. It is true that at the end of last year I purchased Jackson's gastroscope and made examinations with the instrument in the deadhouse; and more recently when performing oesophagoscopy on the living subject I have on a few occasions been unable to resist the temptation to go beyond my own field of practice and trespass on the territory of the general physician and surgeon by passing a tube into the stomach. My claim, however, to be in a position to criticize with confidence some of the statements, and the instrument of Drs. Souttar and Thompson, rests mainly on my not inconsiderable experience as an oesophagoscopist. I recognize that these investigators are on the right track, and I can assure them that these criticisms are offered in no ungenerous spirit.—I am, etc.,

London, W., Oct. 2nd. WILLIAM HILL.

SIR,—Dr. William Hill very kindly informs me that he has written to you pointing out the work which has already been done on gastroscopy. In the *Journal of Medicine* of July, 1908, will be found a paper by Dr. Thompson and myself, in which is given a full account of previous work, with references to all that has been written on the subject.—I am, etc.,

London Hospital, E., Oct. 4th. H. S. SOUTTAR.

THE CRESCENDO MURMUR OF MITRAL STENOSIS.

SIR,—The view lately put forward by Dr. Brockbank that the so-called presystolic murmur is ventricular systolic in origin, which has already been opposed by Sir J. F. H. Broadbent and by Dr. Frederick Taylor, seems to me likely to continue to be periodically revived so long as the relative thickness of the auricle and ventricle, and their apparent relative strength, inordinately impress the mind.

As a matter of fact the auricle is not to be despised, especially in mitral stenosis cases. About fifteen years ago, in letters to the BRITISH MEDICAL JOURNAL and *Lancet*, I discussed this question. I then showed that dilatation of the auricle was a late phenomenon in mitral stenosis; thus at autopsies at Guy's Hospital on cases from the surgical wards in four years 18 cases of mitral stenosis were found. In these death occurred from surgical causes and not from heart failure, except in one case where the patient died chiefly of his heart affection, with thrombosis and gangrene. The latter was the only instance among the 18 in which auricular dilatation was noticed.

A second factor, which I believe will have to be admitted, is that in mitral stenosis cases the contraction of the auricle is generally prolonged. This would appear probable *a priori*, because the auricle cannot empty itself so rapidly through a small orifice as through a large one, with the result that the contraction of the ventricle begins before that of the auricle ceases.

Roy and Adami, in the *Practitioner*, vol. xliv, p. 251, published a set of curves showing graphically the relation in time of the contraction of the auricle, the ventricle, musculi papillares, etc, as well as the relative period of the apex beat, outflow from the ventricles, etc. According to their tabulated results, the auricular systole not only commenced before the ventricular but overlapped it, and, in fact, did not cease till after the aortic valves opened. The examination of many traces published by Dr. Galabin and others confirm this view, especially for cases of mitral stenosis.

A third and much overlooked factor in the cardiac circulation one must admit *nolens volens*. It is that a contracting chamber can exert ever-increasing pressure on its contents as its size diminishes. If the force of contraction remain constant, the pressure on its contents increases in proportion as the diameter of the chamber decreases. Thus, if the diameter become one-fourth of what it was originally, the internal pressure will augment fourfold. If the chamber become completely contracted, having a diameter zero, it would require infinite pressure to open it up or dilate it from within. In the same way the pressure in a powerful motor tyre cannot burst the little rubber pump-tube through which it is blown up, nor the pressure in a thick-walled steam boiler burst or dilate up a thin little copper tube leading from it.

Inasmuch as the systole of the auricle commences before that of the ventricle, the auricle is decreasing in size, while the ventricle which receives its contents is increasing. It is physically probable, therefore, that when the outflow from the auricle into the ventricle is somewhat obstructed, the contents of the auricle which leave later are projected into the ventricle with ever-increasing force, and a crescendo murmur under such circumstances might well be expected.

In cases of slight stenosis the auricle might succeed in emptying its contents into the ventricle before the latter commences to contract. As the stenosis became more pronounced it would not quite succeed, and the ventricle would try to force blood back into the contracting auricle while the latter was endeavouring to send its residual blood forward into the ventricle. In such a contest the auricle would probably be the better man, for it would be small and contracted when the opposing ventricle was full and big. By its very smallness at the time it could oppose the ventricle and quite possibly still send blood onward into it, just as a small, thin, inflated indiarubber balloon connected with a large thick one, similarly inflated, will blow its contents into the latter and further inflate the thick balloon.

The auricle in cases of compensated mitral stenosis is the natural defender of an incompetent mitral valve. A contracting, full ventricle could not cause regurgitation while the auricular systole continued, for it would be physically impossible for it to dilate up a contracted auricle whose chamber at the time has a diameter several times smaller than its own. If the ventricle when full could not overcome the contracted auricle it could not later, for it has a safety valve at the aortic orifice which prevents its internal pressure rising appreciably as it decreases in size.

In the earlier stages of mitral stenosis we have therefore, as a rule, what we might expect—namely, a crescendo auricular-systolic—the so-called "presystolic"—murmur, and no ventricular systolic murmur.

As the malady progresses and the auricle fails to sufficiently expel its contents to become adequately contracted before the ventricular systole commences, it cannot resist that systole. Regurgitation will then occur through an incompetent mitral valve, and a systolic murmur will probably become audible. The auricle will now begin to dilate and compensation seriously break down. With rest and treatment the auricle may recover for a time its tone and function of blood propeller and valve defender, which to me explain Dr. Taylor's statement: "The presystolic murmur returns when cardiac efficiency is temporarily improved or restored."

I have said nothing about auricular hypertrophy. It occurred in only 3 of my 18 surgical cases, and obviously only develops when required to overcome increasing resistance. In 70 autopsies at Guy's Hospital, mitral stenosis cases during the four years 1888–91 which I analysed, severe stenosis (one finger or less) occurred in 36 cases. In these severe stenosis cases dilatation of the auricle, without simultaneous hypertrophy, occurred in only 3 instances. In 11 cases dilatation and hypertrophy occurred together, and in 15 hypertrophy was alone noted.

It is evident, therefore, that in mitral stenosis hypertrophy of the left auricle is a much more characteristic phenomenon than dilatation. The advantage the auricle, therefore, has over the ventricle through commencing its contraction earlier and becoming small is supplemented in many cases by a useful hypertrophy of its walls, which we know is frequently accompanied by a decrease in the thickness of the left ventricle.—I am, etc,

Topsham, Oct. 4th. D. W. SAMWAYS.

FASTING PRISONERS AND COMPULSORY FEEDING.

SIR,—I cannot help expressing my great regret that an article so unfair as that on the compulsory feeding of fasting prisoners should ever have appeared in the BRITISH MEDICAL JOURNAL.

Whatever those women may have done in the way of violence (and it is as nothing in comparison with what has been done in similar circumstances by men, from Irish Home Rulers to Dr. Jameson), the essential cause of their imprisonment is that they are fighting for a political idea. Even the Government, though it will not treat them as political prisoners, does not venture to deny that. For this they are being treated as common criminals in a way that men never are, and forcible feeding is resorted to because that is the only way in which the Government can make the continuance of their punishment as common criminals possible.

Whether six days' complete deprivation of food is accurately described as a "cheap way of escaping the penalty of the law," I am not competent to judge (has the writer of the article tried it?); but there can be no question that to take women, even when enfeebled by prolonged fasting, hold them down by force, and pass a tube down either their nose or their mouth, in spite of their efforts to resist, is both an outrage and dangerous, and that Mr. Keir Hardie rightly described it as such. Forcible feeding may be necessary at times in a lunatic asylum. Here it is not necessary, except to gratify a Government that is vindictive because it has made a mistake and has not the moral courage to acknowledge it.

There are instances on record in which under similar circumstances, when there has been resistance, food has passed into the lungs or the oesophagus has been torn. Should anything of this kind happen here, it would come perilously near manslaughter or even something worse.

I notice that Mr. Masterman now is advised that no 'serious injury is to be apprehended" in any case of this kind. I do not know by what standard his adviser judges,

but evidently the infliction of some injury is to be regarded with equanimity.—I am, etc.,

London, W., Oct. 5th. C. Mansell Moullin.

Sir,—I fear that you can hardly be exonerated from showing a certain amount of bias in your paragraph on the above subject in the British Medical Journal of October 2nd.

As to the merits of the case at issue between the so-called hunger-strikers and the present Government I am not here concerned. But we look to you not simply to give us information on the medical side of matters of public interest, but also to indicate the attitude that should be taken by the medical profession towards any matters of public controversy.

Now it appears to me that you have unintentionally ignored an aspect of this forced-feeding controversy which is to me, as a medical man, of the first importance. Do you realize that, by what you say in your short article, you are placing the prison medical officer in a most unenviable position?

I always imagined that the duties of such medical officer involved the treatment of disease and were practically confined to that. By implication, you now support the view that he should also assist the lay authorities in dealing with recalcitrant prisoners. If these prisoners decline to take food let them be suitably punished as they would be for any breach of prison regulations. But if any forcible treatment is to be adopted which the administrative staff of a prison may think necessary, let them be required to do this themselves and at their own risk rather than make use of the professional assistance of the medical officer, upon whom will rest the blame of any accident. And in the forcible introduction of soft rubber tubes this is not remote.

I personally regard it as in the highest degree derogatory to the dignity of the medical profession that its members should be called in to treat with force healthy but recalcitrant prisoners. There is a suggestion in this of the status of the profession being removed only in degree from that of the common executioner or flogging warder. For this reason I view with disgust the employment of the medical officers of prisons to deal forcibly with rational prisoners who decline to eat, and I trust that, if the practice of hunger-striking becomes general, a majority of the prison medical officers will decline to give their services except in urgent cases.—I am, etc.,

Northwood, Oct. 5th. J. S. Edkins.

Sir,—I should like to criticize some statements which appeared in the last issue of the Journal on the compulsory feeding of the suffragist prisoners. The writer says that the dangers of the proceeding are so slight as to be negligible. May I point out that there is considerable divergence of opinion as to the danger of artificial feeding even under the familiar conditions of hospital and asylum practice? The circumstances under which forced feeding is applied to the prisoners are not matters of experience; new factors are present; the proceeding is in the nature of an experiment, and no one is in a position to dogmatize on the extent of the risk which is run. The prisoners are worn out by previous starvation; their hearts, owing to deprivation of food, are in a weak condition; and, instead of acquiescing, they are determined, rightly or wrongly, to resist so that they can only be fed after they have been overpowered by superior physical force.

The "hunger-strike" was not instituted as a cheap way of escaping imprisonment, but as a protest against treatment as common criminals. If the Government recognized these women as political offenders, and placed them in the first division, the need for coercion would cease.

In the Birmingham prison the stomach tube is not employed in order to save life according to its normal use as a surgical instrument; it is not employed in order to prevent the condemned from escaping imprisonment, but in order that political prisoners may be coerced into accepting the status of ordinary malefactors.

It is also a misnomer to describe complete starvation, up to six days' duration, as a "cheap" means of escape. The writer, if he speaks from personal experience, must possess unusual powers of endurance and fortitude. To ordinary people the suffering which a "successful" hunger strike involves is very great, and it might well be regarded as an adequate punishment for any of the offences with which these ladies are charged.—I am, etc.,

L. Garrett Anderson, M.D., B.S.Lond.

London, W., Oct. 6th.

⁂ We cannot agree that "the proceeding is in the nature of an experiment." Many prisoners, including, we believe, Palmer, the Rugeley murderer, have tried to starve themselves, and have been prevented from doing so by compulsory feeding.

CLINICAL TEACHING.

Sir,—The correspondence in your columns, under the above heading, confirms a widely-held belief that clinical teachers have much to learn—about teaching. As a would-be clinical teacher I feel sure I should only add proof to this belief if in this communication I attacked existing methods or submitted to your readers various schemes of reform which I regard as desirable. I am keenly conscious of the fact that I tumbled into my post as a clinical teacher in the same haphazard manner as did most of my colleagues, and I am therefore not surprised that few of us know much of the great principles of education which are as applicable to professional training as to the acquirement of all forms of knowledge. Our great British Medical Association has done, and will continue to do, invaluable work in diffusing medical knowledge throughout the profession. It might, I think, add to its services by helping our teachers to learn how to teach. For this purpose I would suggest that the proper authorities should consider the question of instituting a section of Medical Education at our annual meetings. On no occasion in the year are more English-speaking clinical teachers brought into contact with each other. Their deliberations would be helped by the criticism of some of their most distinguished foreign *confrères* and, what is of still more importance, by hosts of general practitioners who, having suffered from, or benefited by existing methods, would be able in their own persons to demonstrate the results.—I am, etc.,

October 4th. A Clinical Teacher.

THE POOR LAW REPORT AND THE SANITARY MEDICAL SERVICE.

Sir,—Your correspondent, Dr. Greenwood, talks of the "apotheosis of the sanitary medical service," and asks where the Poor Law medical officer comes in. He does not come in. Apparently, men who have been specially trained in diagnosis and treatment are to give place to sanitary officials who have been trained, and have since practised, on totally different lines. Why not adopt the "Mad Hatter's" tea party of *Alice in Wonderland* at once, and turn obstetricians into ophthalmic surgeons, and laryngologists into rectal surgeons?

As regards the Minority Report, it is pure Socialism, and sets up a system of petty officials throughout the land, who will be employed to look after each other and see that his fellow official performs his duties aright; each one, apparently, is to be paid out of the taxation of his own salary. Another instance of a community getting its livelihood by taking in each other's washing.—I am, etc.,

Wrington, Oct. 4th. Hubert C. Bristowe.

Sir,—I shall be glad if you allow me space to point out that the recent resolution of the Representative Meeting not only affirms the desirability of medical officers of health *not* being engaged in private practice, but is couched in terms which carry with them an affirmation of the undesirability of medical officers of health holding education or hospital appointments, or indeed doing any work outside the "duties of their office."

The Representative Meeting has, in fact, then already adopted a policy which, so far as medical officers of health is concerned, goes beyond the lines of the Majority Report. The day of the "combined district" is, as we all know, passing. The policies of the Representative Meeting and the Majority Report will lead, as many of your correspondents foresee, to a multiplicity of small "whole-time" appointments at £250 a year or so each. Is this in the interests of the public health? It certainly is not in the interests of the profession.—I am, etc.,

F. G. Crookshank,
Medical Officer of Health Urban District of Barnes.

London, S.W., Oct. 1st.

COURT-MARTIAL ON A FLEET SURGEON.

SIR,—As a member of many years' standing of the British Medical Association I am concerned in the views represented by its JOURNAL. I write with reference to the general tone of the article appearing in the BRITISH MEDICAL JOURNAL of September 25th, which discusses the recent court-martial on Fleet Surgeon C. G. Matthew. This article conveys a wrong impression of the existing relations between the medical officers and the executive or other officers of His Majesty's navy. Friction is quite unusual, and mutual courtesy quite general.

The authorities were possibly ill advised in allowing the petty charges against Dr. Matthew to be brought before a court-martial. However that may be, the result itself and the evidence given by the executive officers concerned prove that such courts-martial are at least fair and free from class prejudice. The records of courts-martial for several years past can show no parallel case.

The reference to "self-respecting naval surgeons" in Mr. Gerald Sichel's letter of October 2nd is scarcely courteous to such doctors as make an efficient naval medical service one object of their lives. His remarks are untrue to actual conditions, however unfortunate his own somewhat limited experience may have been in the past.

You will understand, Sir, that I have thought it right to add these comments to those of "Another Fleet Surgeon," having regard to the wide circulation of the BRITISH MEDICAL JOURNAL amongst all classes, and to the general reliance on the correctness of the views it expresses.—I am, etc.,

October 5th. AN AVERAGE FLEET SURGEON.

THE STRENGTH OF DIGITALINS.

SIR,—We agree with the opinion of Dr. F. C. Eve (expressed in his letter in the BRITISH MEDICAL JOURNAL of September 25th) that the nomenclature of the various active principles of digitalis must be very confusing to the mind of the busy practitioner who cannot find the time to make a special study of this intricate problem. The confusion is accentuated by the fact that the most active of the constituents of digitalis has received the designation digitoxin, implying a harmful, rather than a beneficial action.

According to Schmiedeberg, digitoxin is a crystalline principle quite insoluble in water, having a characteristic colour reaction, and (we quote from Squire's *Companion to the British Pharmacopoeia*) "most toxic of all the constituents (of digitalis), but uncertain, cumulative, and dangerous in its action."

Professor Cloetta discovered a glucoside with an identical reaction, but amorphous and soluble in water. It is stated to be non-cumulative, certain and safe in its action. "Digalen," which was brought to the notice of Dr. F. C. Eve in Switzerland, is a solution of amorphous digitoxin Cloetta. Its standardization (0.3 mgr. in 1 c.cm.) is designed to make 1 c.cm. correspond to 0.15 gram of folia digitalis *B.P.*, or 20.3 minims of tr. digitalis *B.P.*

In regard to digitalin we are unable to add to the information already published beyond the fact that four distinct varieties occur in commerce.

The very wide and much debated question as to the admissibility of results obtained by animal experiments to direct application to human therapeutics we are not in a position to decide. Nevertheless, we may perhaps call attention to the publications of Dr. Minder,[1] Dr. Hipp,[2] and Dr. Kumoji Sasaki.[3]—We are, etc.,

THE HOFFMANN-LA ROCHE CHEMICAL WORKS, LTD.

London, E.C., Oct. 2nd.

[1] *Berl. Tierarztliche Woch.*, 1909, No. 11.
[2] Dissertation from the Pharmacol. Inst. of the Royal Vet. Academy, Hanover.
[3] *Berl. klin. Woch.*, 1905, No. 26.

AN exhibition of foods, drugs, pharmaceutical preparations, and medical and surgical appliances, organized by the *British and Colonial Druggist*, has been in progress at the Royal Horticultural Hall, St. Vincent Square, this week, and will remain open until 7 p.m. on Friday, October 8th.

THE PLAGUE.

PREVALENCE OF THE DISEASE.

INDIA.

FROM January 1st, 1909, to August 28th, 1909, the deaths from plague in India amounted to 90,000. Distributed in months the returns show that in January there were 9,829 deaths from plague, in February 13,075, March 18,258, April 21,285, May 16,050, June 3,478, July 2,649, August 5,376. The returns are satisfactory, for practically the 90,000 deaths represent two-thirds of the expected amount for the year; so that we may anticipate that the total returns for the year will not exceed 135,000, or just under those of 1908, when 148,700 deaths were recorded. These are, however, eminently satisfactory compared with 1907, when not fewer than 1,316,000 persons died of plague. The Punjab has been the most seriously affected district, for out of the total of 90,000 since January 1st to August 28th no fewer than 23,867 occurred in that province.

HONG KONG.

The deaths from plague in Hong Kong in April, 1909, numbered 19; in May, 34; in June, 35; in July, 12; in August, 4. On August 28th Hong Kong declared free of plague.

CHINA.

In Canton, Amoy, Swatow, and Pakhoi plague has prevailed in varying degrees of intensity since March to the present date. No statistics are available.

JAPAN.

Plague cases have been reported during the spring and summer months from Kobe-Hiogo, Osaka, Yokohama, Himeji, Sugahama. In Kobe-Hiogo alone has the outbreak been serious.

FORMOSA.

In the provinces of Tainan, Kagi, and Yensuika plague was reported in the spring of 1909.

SINGAPORE.

During March and April, 1909, 12 cases of plague were reported from Singapore.

PERSIA.

In June, 1909, 6 cases of plague occurred in Bahrein.

TURKEY IN ASIA.

At Djedda 67 deaths occurred between February 9th and March 7th, 1909; at Bagdad, between May, 1908, and May, 1909, 163 cases of plague were reported.

EGYPT.

Between March 13th and July 9th, 1909, 125 deaths from plague were reported from Egypt. The disease occurred chiefly in Aga, Tantah, Deirut, Mineh, and Mallawi.

BRITISH EAST AFRICA.

In June 4 cases of plague reported from Kisumu.

GERMAN EAST AFRICA.

In April 80 deaths occurred at Buhungukirra, and during April and May at Muansa 12 cases were reported.

ZANZIBAR.

During the summer of 1909 plague occurred in Zanzibar, and 1,731 cases were inoculated with Haffkine vaccine.

MAURITIUS.

During April, 1909, 5 deaths were reported; in May, 4; in June, 4; in July, 6; in August, 10; and in September (to 18th), 11.

AUSTRALIA.

Queensland.—From January 1st to June 30th, 2 deaths from plague were reported.

South Australia.—At Adelaide, in April, 2 cases of plague occurred; in May 1 case, and in June 1 case.

New South Wales.—At Sydney, 18 cases of plague reported during March, April, and May, 1909.

West Australia.—In March, 1909, 2 cases reported from Bunbury.

WEST INDIES.

Trinidad.—During June and July 4 cases reported from Port of Spain.

SOUTH AMERICA.

Brazil.—At Rio de Janeiro, during 1908, 53 persons died of plague. During 1909 to June 30th, 17 cases reported.

Chile.—At Mejillones, Antofagasta, Iquique, Caldera, Taltal, and Port of Arico several cases of plague reported.

Peru.—During March 76 cases of plague were reported as having occurred throughout Peru.

Ecuador.—In January, 1909, some 25 cases of plague reported.

Venezuela.—In the first week in June, 1909, 12 cases of plague occurred.

EUROPE.

Russia.—At Uralsk, during July, 1909, 10 cases of plague were reported; the pulmonary type prevailed.

The Azores.—Plague cases were reported at Terceira as late as June, 1909.

Belgium.—Three cases of plague reported on board ss. *Rubens* on April 11th, 1909.

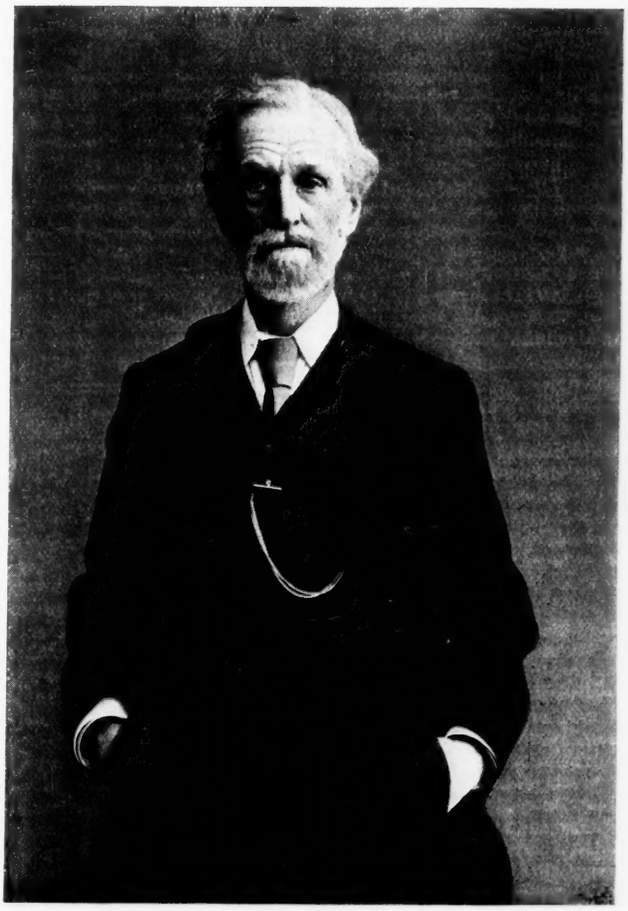
Obituary.

SIR THOMAS SMITH, BART., K.C.V.O., F.R.C.S.,

HON. SERJEANT-SURGEON TO H.M. THE KING; CONSULTING SURGEON, ST. BARTHOLOMEW'S HOSPITAL AND HOSPITAL FOR SICK CHILDREN; LATE VICE-PRESIDENT OF THE ROYAL COLLEGE OF SURGEONS OF ENGLAND, ETC., ETC.

Sir Thomas Smith, who died at his residence, 5, Stratford Place, W., on October 1st, was in his 77th year, having been born on March 23rd, 1833. He was the sixth son of Mr. Benjamin Smith, of Great Lodge, near Tonbridge, Kent. His eldest brother was the late Venerable B. F. Smith, Archdeacon of Maidstone, and his surviving brother, the Rev. Canon Maberly Smith, is rector of Penshurst, Kent.

He was educated at Tonbridge School, where he attained to the sixth form, and thence proceeded to St. Bartholomew's Hospital, where he was apprenticed to the late Sir James Paget, to whom and to whose family he continued to be united by ties of close friendship.



Photograph by] Sir Thomas Smith, Bart. *[Bassano, London W*

After qualifying he held the post of house-surgeon at the Leicester Infirmary, and in a similar post entered upon his long association with the Hospital for Sick Children, Great Ormond Street, then recently founded. At that hospital he became an assistant surgeon in 1861, was promoted to the office of surgeon in 1868, and on his retirement in 1883, after the full period of fifteen years' service in the wards, was elected consulting surgeon.

The interests of that institution were always very near to his heart, and in his later years he often recalled his experiences there. It was this association which led him to take a special interest in the improvement of the operations for hare-lip and cleft palate.

At St. Bartholomew's he held the posts of demonstrator of anatomy and of operative surgery, in 1860 was elected assistant surgeon, attained to the office of surgeon to the hospital in 1873, and was lecturer on anatomy from 1874 to 1879.

He retired in 1898, having reached the age limit of 65 years, and was appointed consulting surgeon and governor of the hospital, in the affairs of which he continued to take an active interest up to the time of his death.

In addition, Sir Thomas was at one time surgeon to the Alexandra Hospital for Children with Hip Disease, and was an honorary consulting surgeon to the King's Hospital for Officers.

On the death of Sir William Savory, Sir Thomas was appointed Surgeon Extraordinary to her late Majesty Queen Victoria, and at the commencement of the present reign was made Honorary Serjeant-Surgeon to the King.

A baronetcy was conferred upon him on the occasion of the Diamond Jubilee in 1897, and in 1901 Sir Thomas was created a Knight Commander of the Royal Victorian Order.

In 1862 he married Ann Eliza, daughter of Mr. F. Parbury, of Lancaster Gate, and had a family of three sons and six daughters, all of whom survive him. Mrs. Smith died in 1879. He is succeeded in the baronetcy by his son, T. Rudolph Hampden Smith, M.B.Cantab., F.R.C.S.,

Sir Thomas Smith took an active part in the work of the various medical societies, many of which are now amalgamated into the Royal Society of Medicine, served the office of Secretary of the Royal Medical and Chirurgical Society, was a frequent attendant at meetings, and contributed valuable papers to the several *Transactions*, as well as to the *St. Bartholomew's Hospital Reports*. His remarks in discussions were always to the point, worthy of attention, and enlivened by that genial humour which was so essential a part of his thoughts and sayings. He could not be wearisome or dull as a speaker.

When the International Medical Congress met in London in 1881, he undertook the arduous duties of the Secretaryship of the Surgical Section.

In the Royal College of Surgeons he occupied a seat on the Council and held office as Vice-President.

In spite of all this work done in and for the profession, he declined the Presidential Chairs which were from time to time offered to him. These refusals arose in part from a diffidence which led him to believe that others would fill them better than he would, and in part from a conviction that the adequate performance of the duties which they involved was not compatible with the claims of his hospital work, and of a large private practice. In the medical and some clerical charities he took an active interest, and took part in the work of their administration. Up to the last weeks of his life he was a regular attendant at the meetings of the Marylebone Branch of the Charity Organization Society, of which he was Vice-Chairman.

Brought up in the country in his boyhood and early youth, he acquired a fondness for active sports which he retained throughout life, and in his holidays, which were often spent in Scotland, he resumed the pursuits of his youth with unabated keenness. He was a golfer before the game had obtained any wide vogue in the South, and up to the early part of the present year was regularly to be seen upon the links at Mitcham. Yet at the dictate of failing health he laid aside without a murmur the active habits of a lifetime.

Of his gifts as an operating surgeon, of his teaching in the wards and in the lecture room, and of his diagnostic insight, it may be left for others better qualified than the writer to speak. It was not as a surgeon alone that Sir Thomas Smith received from a wide circle of friends an affectionate respect and regard such as few men are so fortunate as to inspire. He was a man strictly upright in all his dealings, of deep religious convictions which he never obtruded, humorous with a kindly humour which never wounded, endowed with a fund of sympathy which showed itself in deeds as well as in words.

Many a former pupil will remember the kindly interest which made the allotted task seem lighter. Some who have attained to positions of eminence will recall a helping

hand which made easier the first steps of the ladder. Some with whom life has dealt less kindly will think of him as a true friend in trouble, and all alike will deplore the gap which is left by the death of him of whom they always thought and spoke as "Tom" Smith.

Mr. HENRY T. BUTLIN, D.C.L., President of the Royal College of Surgeons of England, writes:

By the death of Sir Thomas Smith I am deprived at once of an old friend and of one to whom I am indebted for many kindly services in my younger medical days.

When I entered at St. Bartholomew's Hospital he was Junior Assistant Surgeon and Demonstrator of Anatomy, and was exceedingly popular amongst the students on account of his genial kindness and his smartness as a dissector. He was to them "Tom" Smith, and so he remained to a large number of persons to the day of his death. I have no doubt the title pleased him, for I have heard him tell patients who were coming again to see him at the hospital: "Ask for Mr. Tom Smith."

I do not think I had an introduction to Mr. Smith, but he was good enough to show some special interest in me during my student days, and invited me to attend some classes which he held at his house before the final examination for the membership (R.C.S.). As there were no public classes in those days I gladly availed myself of his offer. Well do I remember his delight when he asked a rather dull student when he would take the stitches out after an operation, and the reply was: "When erysipelas sets in, sir."

At a later period I was engaged as registrar and chloroformist to the Children's Hospital (Great Ormond Street), to which he was surgeon, so that I was constantly in contact with him. He was just as popular there as he was with all classes of persons at St. Bartholomew's. The nurses and sisters were all glad when it was his morning to visit the wards or operate. The children adored him. It seems to me only yesterday that I stood by the bedside of a poor little creature whose appetite was very bad, and needed tempting, and Mr. Smith said to the Sister: "What would she like, Sister? Periwinkles, eh?" At which the child's eyes lighted up, and he wrote solemnly on the diet board, "Periwinkles and a PIN." He said, "She must have a pin, Sister, for that's the way they eat them."

About 1882 or 1883 I became his assistant surgeon at St. Bartholomew's, and helped him at nearly all his operations for about nine years, so that I can speak with some authority of him as a surgeon. He was very rapid and dexterous in operating. He was a master of the operation of lateral lithotomy. Both at St. Bartholomew's and at the Children's, people who came to see him perform the operation would sit with watch in hand to see how many seconds would pass between the first incision and the removal of the stone. It was the most beautiful exhibition of rapid and yet sure surgery of that kind that I have ever seen. He was also unequalled in those days in the operative treatment of hare-lip and cleft palate, of which he was a pioneer. Most of the instruments he used were due to his own invention. What a difference there was to be sure in the way he handled those instruments and the way in which they were used by quite excellent surgeons to whom he had himself taught his method of operating!

In diagnosis he was often brilliant, for he had a natural shrewdness and power of observation which stood him in good stead. But I feel sure, in many cases, he could not have stated in words the exact reasons which had led him to the opinion he had formed. I have heard him say, when asked, "Well, I know because I know." AND, the truth is, he DID know. He taught in the out-patient department and in the wards more than had been up to that time the custom, but not to the same extent that such clinical teaching has been carried since. I do not think he lost any of his dexterity up to the time he ceased to be surgeon to the hospital. But he became rather more nervous, especially of loss of blood. On his operating days he was uncomfortable if his assistant surgeon was not there to help him, first myself, and, later, Mr. Walsham. Frequently he would say, "What do you think, shall I do so-and-so?" not because he really was in perplexity, or because he had any intention of accepting our advice, but it became a habit with him.

I suppose many persons who were only superficially acquainted with Sir Thomas Smith regarded him chiefly or solely as an excellent operator, and more or less haphazard in his methods, and certainly not given to literary efforts. This estimate would have been very incorrect; he spoke easily, well, and clearly, and was always welcome at the societies, the Royal Medical and Chirurgical, the Pathological, and Clinical. The indices to the *Transactions* of these societies show what an active contributor he was. He also wrote well and clearly, and furnished a number of excellent papers to the *St. Bartholomew's Hospital Reports*. Those on cleft palate, the acute arthritis of infants, and tuberculous disease of the bladder are well worth reading. There is also a paper on Lister's antiseptic treatment, recording his adoption of it during the three years, 1875 to 1878, and speaking of it in decidedly favourable terms. But he was never an antiseptic surgeon in the sense in which Lord Lister would have interpreted the expression.

It may be readily understood that so able a surgeon, so skilful an operator, and so genial a man, rapidly acquired a considerable private practice. For some years I took charge of it during his absence on holidays. But I looked very young, and the patients would not have me in his place, so I fear I was not very successful as his substitute. I have said that I was indebted to him for acts of kindness years ago. I believe that I largely owed my appointment as Surgical Registrar to St. Bartholomew's Hospital to the manner in which he spoke of my work at the Children's Hospital. He frequently took me with him to administer anaesthetics in order to help me

financially. And his interest continued to the other day, for his letter of congratulation on my election to the Presidency of the College was one of the first and the warmest I received. I might have thought, did I not know otherwise, that his sympathy and kindness towards myself was peculiar and a more or less solitary instance. But I have had many opportunities of knowing that similar sympathy and acts of kindness were extended to other persons than myself, and of learning that his liberality in the case of needy members of our profession was quite extraordinary, seeing how large a family he had and that his fees were never high; indeed, they were not nearly as high as they should have been. This was greatly owing to the manner in which he underrated his own services. I suppose it was the same or a similar feeling which led him to decline offices which would have placed him in a prominent position in his profession. If they entailed exhibition of himself and publicity, he shrank from them, and nothing would induce him to accept them. I have every reason to believe that he might have been President of the College of Surgeons some twelve or fourteen years ago if he had been willing to accept the responsibility. I have no doubt he would have made a good President, for I became a member of the Council long before he left it, and know how excellent a councillor he was. He attended to his duties with great regularity, never spoke unless he had a proper excuse for doing so, and then spoke shortly and to the point. All his fellow-members liked him for the same reasons that he was liked in every place to which he belonged.

Sir Thomas Smith's wit and humour made him acceptable at the dinners at which he dined and the places in which he spoke. I remember how he described a case at one of the societies as that of a little boy whose spleen and liver were contending for the mastery of his abdomen, and certainly they were as large (from amyloid disease) as they well could be; and he introduced another case as that of "a tumour, Sir, to which a little child is attached." The students in the rooms and afterwards in the wards were delighted with his ready wit. Every teacher of anatomy is aware of the industrious but not too sharp student, who, at the beginning of his first session in the rooms, finds a penny in the oesophagus and brings it to the demonstrator in triumph as, perhaps, the undetected cause of death. After this had happened two or three times to "Tom" Smith, he became tired of it, and his advice to the next youth who brought him the penny was, "All right, my boy, you look in the stomach and you'll find the change for a shilling." Going his round of the wards one day, he found a woman in one of them with a badly-cut lip, and asked the cause of it. The lip had been damaged by a jug which had been thrown at her during a dispute with another lady. "How shall I head the board, sir?" said the house-surgeon. "Well, I really think you might head it 'jugged hare lip,'" was the reply.

When I sat down to write this appreciation I tried to tune my mind to the serious part of the character of one for whom I had the greatest admiration and personal liking. But, do what I will, his image comes back to me as I knew him forty years ago: that of a tall man, thin and erect, kindly and good to look upon, with his hat a little on one side of his head, quick and ready of speech, and endowed with the wit which students love, for it was never biting nor ill-natured. And I recall, whether I will or no, the anecdotes which tell of his lighter rather than of his more serious vein. During the last year he seemed to me to have become a little graver, but he had not by any means lost the cheerfulness and brightness which endeared him to us all.

Professor Howard Marsh, Master of Downing College, Cambridge, writes: The announcement of the death of Sir Thomas Smith will be received with deep and universal regret, not only by the profession but also by the public. From the highest to the lowest he was known as a personal friend more widely than, perhaps, any other surgeon of his day, while he was honoured by the confidence and regard of many members of the Royal family, especially by Queen Victoria and their present Majesties King Edward and Queen Alexandra, he was also the friend, in the truest sense, of all his patients at St. Bartholomew's Hospital and the Hospital for Sick Children. It may be truly said that every one loved him. It is during illness that the perception of the character of those around becomes most vivid and unerring. It was a lesson to be present when Tom Smith was at the bedside of a sick child. Take a little girl of 6: there was no trace of fear or shrinking, but a mixture of curiosity and confidence; their eyes met, a little hand was poked out and taken, a wireless telegram seemed to pass, and then there was either a pathetic smile or a coy chuckle, as the case might be, indicating that the doctor had become a sort of favourite uncle or a kind of Father Christmas. Wherever men congregate, in the club or at the coverside, the doings of the prominent surgeons of the day are often the subject of conversation. If any one was speaking hardly of Tom Smith there were sure to be present those who would take the other side. "Ah, I dare say, but he is one of the best. He cured me, and I know he put ——— and ——— right. When you are ill it can't be a bed of roses you know; but he always tried to make it as easy as ever he could. He is a really kind chap." Many a woman after what Tom Smith had done for her, or her child, would have recoiled from any one who spoke against him—there were, indeed, very few who ever did so. He was not only naturally kind, as many are. Kindness is often flabby, and even maudlin, as when a woman degrades a cat by tying a pink ribbon round its neck and undermines its constitution by giving it half a pint of cream at its meals. Tom Smith's kindness was different. It was his answer to the misery of the man whose wife's life was hanging in the balance; to the mother's agony of fear that the life of her only child was ebbing away; and his attitude at such times inspired an unfading memory. No doubt all this was part of his character from the first. But the circumstances of his life had conspired to give it full development. From boyhood onwards he was the intimate friend and constant associate of Sir James Paget, whose kindness of heart was one of his most conspicuous and contagious qualities. And he met with as great a catastrophe as can befall any man, for he lost his wife at the birth of their ninth child. This event changed his whole life, and in a man who had faced such a calamity it was once again seen that a fellow feeling makes us wondrous kind. He was, I need scarcely say, a man of the highest rectitude and the highest ideas. He turned instinctively away from the commercial side of his profession. Till he was nearly 30 he was very poor and was getting his living by coaching and by helping Sir James Paget in his private practice. In those days, I have heard him say, his breakfast was often bread and butter and a penny herring. Throughout his life he lived quietly and limited his expenses very carefully. His wife, a daughter of Mr. Parbury, an Australian merchant, had money, and he had for many years a very lucrative practice, so that he had means sufficient and to spare, although the education of his nine children must have entailed a heavy outlay. He never talked of his money matters, and was content with the motto "*Dabit qui dedit.*" But he was very generous. He was one to whom many a St. Bartholomew's man went when he was in trouble, and, if the case was genuine, he was sure to meet with kindness and sympathy. On such occasions Tom Smith, not only by his own giving, but by getting contributions from Morrant Baker, Alfred Willett, Sir William Church, and others in his set, made up a useful sum, which he took care to see was turned to good account. He also gave freely to the various "friendly" societies, the Sons of the Clergy, Epsom College, British Benevolent Fund, and others, and to St. Peter's Church, Vere Street, where for many years he was a constant attendant when the present Dean of Salisbury, Dr. Page Roberts, was the incumbent. Every one knew how genuinely kind he was, but his popularity was in no small degree due to the fact that he never pushed himself forward, and was always courteous and carefully regardful of other people's feelings, while of their failings, although he was well acquainted with them, he said very little. Sometimes his friends wished he would take a more leading part.

Every one called him "Tom," or "Tom Smith"; this was, to a great extent, his own doing. The name, so completely English and unpretentious, exactly suited one who was absolutely free from affectation. It seemed to amuse him, and he often made fun of it. "Oh, my Tummas!" was his only expletive when he

spilt the ink, pricked his finger, or missed a putt at golf. He had a mechanical turn, and was very good with his fingers, and could mend a fishing rod, patch up a favourite pipe, or contrive and make a splint for any special case with notable skill. Had he started a motor he would have known all about its interior, and been very resourceful in overcoming its ingenious misbehaviour. He was a keen sportsman—a good fisherman, a good shot, and a golfer whom many found it hard to beat. One of the pleasures of my life was our round (or two) together, or in a foursome, for he was always, whether winning or losing, the same genial companion, neither obtruding his good nor explaining away his bad shots—as the manner of some is—and neither doing nor saying anything to rub even a sensitive golfer the wrong way. A gift which was one of the secrets of his popularity was a sense of humour of quite rare quality. He often, with perfect good nature great shrewdness and correct appreciation, hit off a character by some droll comparison that was extremely amusing. He never made a speech if he could help it, but whenever he did, adorned with the virtue of brevity, it always contained some quaint turn or whimsical allusion, or some conceit which would not have been out of place in the pages of Charles Lamb or Laurence Sterne.

Sir Thomas Smith stood in the first rank of the surgeons of his day. He kept to surgery, and although he was awake to all the advances which came through physiology and pathology he did not himself undertake a study of surgery from any but a practical standpoint. He had a very wide knowledge of surgical cases, and all who met him in consultation knew how valuable his opinion was. Here his shrewdness and the close attention with which he observed symptoms and worked out a conclusion gave his opinion great weight. In his early days he was an expert aurist when the majority of surgeons knew nothing of affections of the ear, and he was very successful with many cases which Sir James Paget placed in his hands. It was, however, as an operator that he was most distinguished. I should say, without hesitation, that Sir William Fergusson and Sir Thomas Smith were the best two operators of their day. Neither of them posed as a great operator; they would have been equally ashamed to do so, but their pre-eminence was well known. Certainly Fergusson and Smith had no rivals in the operation of lateral lithotomy in children. In the hands of these two masters a proceeding which many surgeons never undertook without serious apprehension appeared to onlookers to be simplicity itself, for which half a minute was fully time enough. And Smith was an expert in the treatment of hare-lip, in tracheotomy, and in plastic and urethral surgery. He will, however, be chiefly remembered for his work on the operative treatment of cleft palate, for having turned chloroform to account, invented a very ingenious "gag," introduced and improved a French tubular needle for passing silver wire, and substituted lateral incisions for the somewhat uncertain method of relieving tension by dividing the tensores palati muscles employed by Fergusson and others, he was enabled, instead of postponing operation till the patients were 12 or 14, to operate on children of 4, or even 3. Advances have since been made, but these have all been the outcome of Smith's original proceeding.

Sir Thomas Smith was not a prolific writer; he only appeared in print when he had something to say; but what he wrote always attracted attention and helped his subject forward. Many of his papers conveyed important contributions to surgery. That on the acute arthritis of infants may be especially mentioned. For many years his handbook on operative surgery was very popular, not only at St. Bartholomew's, but in other medical schools. It was a matter of great regret that he did not accept the Lectureship on the Principles and Practice of Surgery at St. Bartholomew's in succession to Abernethy, Sir William Lawrence, and Sir James Paget, and as the colleague of Sir William Savory, for as Lecturer on Clinical Surgery he was excellent, and no one was more popular with the class. But as his nine children were motherless and so largely dependent on his personal supervision of their education, as he was in large practice, and as, though his health was sound, he was never robust, he felt he had neither time nor strength for this highly responsible and exacting office. On the same grounds, later on, and with equal regret on the part of all his colleagues, he declined to be elected President of the College of Surgeons. But though he was never one of the systematic lecturers at his school he was for many years one of the most original and instructive teachers of his day. I was long associated with him both at St. Bartholomew's and at the Hospital for Sick Children, and I was constantly indebted to him; I learnt much from him and I always had his constant good colleagueship and friendship.

We know that "men may come and men may go," and that "so flourish these when those have passed away," but in the long succession of men who have been called upon to do their duty to their school, their colleagues, and the public, the name of Sir Thomas Smith will long be held in grateful and affectionate remembrance.

Sir FREDERICK TREVES, Bart., G.C.V.O., C.B., writes Young surgeons of the present day can hardly realize to its fullest extent the loss the profession of surgery has sustained by the death of Sir Thomas Smith. For a number of years, during which time he was keenly engaged in hospital work and busy with a large private practice, he exercised a dignified influence in surgical affairs which was as remarkable as it was unobtrusive and efficient. This influence was due to rare and estimable qualities, among which were conspicuous a lofty ideal of professional honour, a blunt abhorrence of anything that savoured of pretence or unworthy methods, and a clear, simple common sense which was ever of more worth than the acutest sophistry. Sir Thomas Smith was the soul of honour, a man jealous to a degree of the high repute of his profession, who, in every action of his life, realized the conception of the blameless surgeon, and who, by his example, guided many a stumbler through the pitfalls which lie in the path of ultimate success. Many and many a time, in matters of doubtful ethics, Sir Thomas was appealed to with the assurance that his decision, whatever it might be, would be true and right, and that his counsel would be based upon the dictates of a generous and wide-reaching sympathy. In any controversial discussion in which he was concerned he was always the advocate of peace, while, at the same time, his was the bright grasp of facts, his the prompt conclusion, his the magnanimous award. It was enough for Sir Thomas Smith to find a professional brother in difficulties to make him at once his champion, his advocate, and his friend. He would go far and travail long to do a kindly deed. His unselfishness, his modesty, his gentleness were everyday unconscious expressions of a great spirit, and will ever serve to illumine the records of the life of one of the most lovable of men. He is known to the world as a great surgeon, he is known to the many as a noble and upright man, but I believe that it would best please his gentle heart to think that to a still larger host he will ever be known and affectionately remembered as simply—"Tom Smith."

Sir Thomas Smith was buried at the St. Marylebone Cemetery, Finchley, on Wednesday, October 6th. The funeral service was conducted by the Bishop of Oxford, and was attended by his children and some other members of his family. A memorial service which was held simultaneously at St. Mark's Church, North Audley Street, was conducted by the Dean of Salisbury, who was assisted by the Rev. H. J. Watters. At this service His Majesty the King was represented by Sir Frederick Treves, Bart., G.C.V.O., and there was present, in addition to the Presidents of the Royal College of Physicians and Royal College of Surgeons, a large gathering of relatives, private and professional friends, and representatives of the various institutions with which Sir Thomas was connected.

JOHN MILSON RHODES, M.D., J.P.

THE circumstances of the death of Dr. John Milson Rhodes, which occurred suddenly on Saturday evening, September 25th, at his house, Ivy Lodge, Didsbury, were peculiarly sad. For over three years he had suffered from an incurable heart affection, and on his own accord, as well as under medical advice, had resorted to full doses of nux vomica or strychnine to enable him to cope with his work. Having much Local Government Board work to do, on that Saturday at 5 p.m. he took mx of liq. strychninae and resumed his writing. At 7.30 he felt the

muscles of his limbs twitching, and sent for his nephew, Dr. M. Russen Rhodes, who was in partnership with him. He appeared cheerful, but remarked: "You do not think I have taken a fatal dose?" He was reassured on that point; nevertheless various restoratives were given him. He apparently recovered, and spoke of the public work he had to perform during the following week. Within the next hour he had several relapses, and though strenuous efforts were made to counteract his now visible heart failure he collapsed and died shortly after nine o'clock. It was the end that both he and his nephew had expected for some considerable time, for on the occasion of a projected trip to Budapest to attend a conference he remarked that "probably he might not go, as it would give so much trouble had he to be brought home."

Milson Rhodes, as he was known, was born in Broughton, Manchester, in 1847. He was one of a family of doctors whose name and fame have for years held a dignified place in that character in South Manchester.

His two older brothers, Dr. Francis Rhodes and Dr. Thomas Wemyss Rhodes, predeceased him by many years. Deciding to enter the medical profession, he matriculated at Glasgow University, but completed his studies at Owens College and the Manchester School of Medicine, where he distinguished himself as a first and second prizeman with honours. After qualifying at Edinburgh and graduating at Brussels, he settled at Didsbury in general practice, and had resided there ever since.

But the reputation of Milson Rhodes was far more than local. Indeed, on certain burning topics, like Poor Law administration and, in a special sense, all the conditions relating to epileptics and the feeble-minded, as well as the question of infant mortality and its causes, he earned a reputation that might almost be termed world-wide.

His work has been that of the pioneer, and, though many so-called authorities in his earlier days scoffed at his ideals, he had the supreme satisfaction of living to see the fruition of nearly every one of his aims in the most practical form.

Photograph by] JOHN MILSON RHODES. [*A. Coupe, Manchester.*

It was chiefly under his influence and lead that Manchester became the forerunner and centre of reform in many phases of workhouse management. After serving for two years as an overseer for the township of Didsbury, Milson Rhodes in 1882 became a member of the Chorlton Board of Guardians, and was paid the unusual compliment of being invited to serve in this capacity by both political parties—Liberals and Conservatives. The satisfaction his place on the board gave to all is shown by the fact that never since had he to fight for his seat.

His first discovery as a guardian was that the workhouse dietary system was in a state of chaos. In some places, for instance, the inmates were allowed 3 oz. of meat for dinner, in others 6 oz.; in one workhouse the amount of suet pudding allowed per head was 8 oz., in another 24 oz. Dr. Rhodes brought the matter before the North-Western Poor Law Conference, and as an outcome of the discussion alterations were made in many unions. It was when, as a guardian in 1884, he discovered that in the Chorlton Union a ton of bread per month was being wasted, that he wrote a paper on workhouse dietaries, a paper so trenchant in its criticism of the prevailing system that in the end the Local Government Board was forced to take up the subject with the result that dietary scales on a scientific basis were prepared by Dr. Downes and are now in use all over England.

Sick nursing was his next subject. In those days many workhouses had no trained nurses, the duties being relegated to pauper inmates who were open to all sorts of bribery. With his usual vigour he directed the attention of local administrators in the first instance to the need for skilled nursing in the sick wards. His paper on trained nurses in workhouses he then read before the conference, and soon afterwards the Northern Workhouse Nursing Association, of which he became chairman, was established. Matters progressed, and the law now compels the employment of trained nurses in hospitals for the pauper sick.

Much anxious time, freely given, and earnest work have been devoted by Milson Rhodes to the condition of the epileptic and imbecile poor in workhouses, and his labours in this direction alone were most strenuous. So long ago as 1888 at a meeting of Poor Law representatives in conference in a paper on pauper lunatics, their detention in workhouses and county asylums, and the laws relating thereto, he advocated the complete removal of those afflicted from the ordinary institutions, and showed the harsh conditions under which the mentally deficient who happened to be poor were forced to live. Portions of his paper were quoted in the report of the Royal Commission on the Education of the Blind and Deaf, and some of his recommendations were embodied in the Act that was subsequently passed.

Some years later he proceeded to Germany and Belgium and examined their outdoor or "colony" system of treatment, which was afterwards described in a valuable report. No less important was his report of his tour of inspection of nearly twenty hospitals and colonies in the United States of America, published in 1902, his object being to obtain information as to the treatment of epileptics in the States, as well as knowledge germane to the subject in other respects.

It was due principally to his continued energy that joint action was taken by the Chorlton and Manchester Boards of Guardians to provide such an institution, for he held strongly "to the belief that the most effectual method of dealing with this afflicted class (the epileptics) is to keep them employed as much as possible in the open air, and that the routine of the poorhouse places restrictions in the way of any improvement of which their condition might be capable. At the same time, their presence among the other inmates is also detrimental to them." Milson Rhodes knew the value of "try again," and generally got his result; the result in this case was the establishment of the Langho Colony for Epileptics on a site occupying 168 acres. He laid the foundation-stone in 1904, and

the institution was opened by the late Earl of Derby in September, 1906.

Prior to this it was his advocacy of the cause of the epileptics that led to the foundation by the Lewis Trust of the David Lewis Epileptic Colony at Sandlebridge which cost £100,000, and which he always asserted is the finest institute of its kind in the world. This he accomplished after first experiencing the most determined opposition to his scheme, but he afterwards had cause to rejoice that its example was followed by many boards throughout the kingdom.

Years earlier his endeavour to secure better advantages for feeble-minded children had led to the foundation of the Birmingham Homes for the Feeble-minded, and of these he was appointed the first president.

Dr. Rhodes's work on behalf of poor children might alone suffice as an epitaph for most men. Frequently did he advocate the removal of children from the surroundings of the workhouse, and it was largely through his instrumentality that the Chorlton Board of Guardians established the excellent cottage homes at Styal in Cheshire under the supervision of skilled foster parents. To obtain this reform of taking children out of the workhouse environment he travelled all over the country addressing Poor Law conferences in all the Local Government districts, as well as many other meetings. For some years he was at frequent intervals invited to address meetings on this subject, and no one responded more readily to the general call, for the matter was dear to his heart.

Milson Rhodes also took an active interest in the work of the Lancashire Asylums Board and in the inquiries of the Manchester Statistical Society into the Poor Law. He also represented the Withington district on the Lancashire County Council, and spent as much of his time as could be spared from Poor Law work in dealing with sanitary questions.

On the ever-present question of poverty his fame as an authoritative writer and speaker spread, and he was invited to address congresses in Austria, France, and Belgium on its various aspects. Before the British Association in 1890 he dealt with Pauperism, Past and Present, in a paper that was translated into French.

He was a most prolific writer, and as a statistician on the subjects that constituted his life's work he had no equal. Among his papers and reports, which are legion, are the following:

The Treatment of Pauper Imbeciles and Epileptics by Combination of Unions; Provision for the Insane in the North-western District; Is Insanity Increasing? The Increase in the Number of the Certified Insane, and the Decrease in the Mortality from Diseases of the Nervous System; The Mentally Defective in Prison; On the Necessity for Increased Provision for the State Insane; On the Best Means of Providing Accommodation for the Imbecile and Epileptic Classes; The Treatment of the Mentally Defective under the Poor Law; On the Employment of Trained Nurses in Workhouses; The Progress of the Poor Law during Her Majesty's Reign; The Local Government Bill as it Affects the Functions of Guardians in Urban Districts; Outdoor Relief; Classification of Workhouses; Workhouse Infirmaries; Pauper Statistics and the Case Paper System; The Law in England for the Protection of Children under the Care of Guardians; Report of the Departmental Committee on the Education of Dependent Children; The Necessity for Increased Power of Detention in regard to Immoral "In and Out" Cases and their Children; Charity True and False; Women's Work and Wages; How Far Water is Responsible for Disease.

The number of official appointments he held at various times were many, amongst them being Justice of the Peace and Alderman of the County Palatine of Lancaster; Chairman of the Chorlton and Manchester Asylums Board; Visiting Justice for His Majesty's Prison, Strangeways; Chairman of the Central Poor Law Committee of England and Wales; Chairman of the Chorlton Board of Guardians; Chairman of the Styal Cottage Homes; Vice-Chairman of the Lancashire Inebriates Acts Board; member of the Committee of the David Lewis Manchester Epileptic Colony; and Chairman of the Manchester South Division of the British Medical Association. He was also one of the founders of the Société International d'Assistance Publique, of which he was a vice-president.

His friends often marvelled that he contrived to do so much hard work in the time at his disposal, for he was also medical inspector under the Factory Act. The secret lay in the regularity of his mode of living. He never appeared to be in a hurry, though his average day's work might well have appalled the heart of a man thirty years his junior; he had a time set apart for every detail of his daily life, and allowed nothing to interfere with it. In his own township "the doctor" was in almost daily request for every public movement that was afoot, and he was ever ready to lend his presence and influence in any and every legitimate cause of charity, as well as in every social phase of village life. Even in local games of lacrosse and cricket he spared time to cheer on the younger members on the field by his genial presence and encouragement.

Dr. Rhodes was unmarried, but his loss is mourned by the whole people, and especially by the very poor, whose champion he had been throughout his entire life. As the Manchester city coroner truly observed:

We all know how he lived his life for other people, and it appeared that he had sacrificed it to cope with that work.

Even in the imbecile wards of the Chorlton Union Workhouse Milson Rhodes knew every inmate not only by headmark but also his life's history to the most minute detail. He was essentially a modest man where anything affected himself personally, but a most strenuous and fearless fighter for the rights of others, especially for the weak and afflicted, and knew humanity so well that he was a born leader of men.

His figure was familiar in the lobbies of the House of Commons when any Bill was under discussion in which he was specially interested. It was said that on one occasion he button-holed a consequential M.P. who had a bill to engineer. Milson Rhodes explained to him that such a thing would have to be prominently brought forward before the House and passed by it. "But my dear doctor, it could not be done," said the M.P. "Could not be

done!" cried Rhodes, looking aghast, "my dear sir, I did not come here to discuss the question with you as to whether it could or could not be done; it will *have* to be done." "Oh, if that's how you speak, doctor, you had better come in here, and we'll have a talk over it and see how it can be managed." This was typical of the man—his earnest personality, his sound judgement, and complete mastery of detail carried the day.

An impressive funeral service was held at Emmanuel Church, Didsbury, on Wednesday, September 29th, which was largely attended by his personal friends and deputations from various public bodies. Cremation followed at the Manchester Crematorium. One of the greatest and most eloquent tributes to his character occurred on the way to the Crematorium when a number of aged pauper men from the workhouse were seen standing with heads uncovered and with visible signs of emotion plainly written on their faces.

It has been written of him with truth—

Milson Rhodes's name will live. The good he has done will not die with him, and generations yet to come will have reason to remember him gratefully.

And we might also add "lovingly."

"Wire-pulling" on his own behalf was alien to Milson Rhodes's nature, and if he did not meet with many of the outward earthly rewards for his unselfish labours in the cause of suffering humanity, we know of no more fitting quotation that could be said of him than: "Well done thou good and faithful servant: thou hast been faithful over a few things, I will make thee ruler over many things: enter thou into the joy of thy Lord."

Universities and Colleges.

UNIVERSITY OF LONDON.

Advanced Lectures in Physiology.

The first of the course of eight lectures during the first term in the Physiological Laboratory, by Professor G. A. Buckmaster and Mr. J. A. Gardner, will be given on October 12th, at 5 p.m., the subject being recent researches on chloroform anaesthesia.

Professor E. H. Starling's course of eight lectures on recent advances in the physiology of digestion, will begin on Friday, October 15th, at 5 p.m.

The course of six lectures on the pineal and pituitary bodies, by Professor A. Dendy and Professor W. D. Halliburton, at King's College, commences on Monday, November 1st, at 4.30 p.m.

Lectures by the Professor of Protozoology.

Professor E. A. Minchin will give a course of fourteen lectures on protozoan parasites, with special reference to those of man at the Lister Institute of Preventive Medicine on Mondays and Thursdays, at 5 p.m., during the second term (January to March). These lectures, which are free by ticket, will be addressed to B.Sc. students, medical men and others interested in the subject, and will be followed by exhibits of preparations as occasion requires.

THE UNIVERSITY OF BELFAST.

Proposed Lectureship in Scholastic Philosophy.

The Marquess of Londonderry and others have presented a petition to the Lord Lieutenant of Ireland against the establishment of a lectureship in scholastic philosophy in the university. One of the reasons advanced is that at a public meeting to further the Better Equipment Fund of the college the most explicit promises were made by the authorities that no attempt would be allowed to introduce any sectarian education, and that in reliance on these promises, funds were subscribed, and that the petitioners consider a lectureship in scholastic philosophy is sectarian.

Three petitions have also been presented to the Judicial Committee of the Privy Council, and a preliminary hearing took place, but was adjourned; and the full hearing will be held on October 14th. Both petitioners and the University Commission will be represented by counsel.

The matter, although not strictly medical, is arousing the deepest concern in all circles. Many feel strongly that indirectly the interests of the Belfast Medical School are vitally involved in the issue.

THE UNIVERSITY OF BIRMINGHAM.

Appointments.

Professor Jordan Lloyd has accepted the invitation of the Council of the University to succeed Professor Bennett May as Professor of Surgery.

The vacancy caused by the resignation of Professor Robinson, who was Professor of Anatomy, has been filled by the election of Mr. Peter Thompson, M.D., Ch.B.Vict., Professor of Anatomy in King's College, London.

The Chair of Zoology has been filled by the appointment of Mr. Frederick William Gamble, D.Sc., F.R.S., who was Assistnat Director of the Zoological Department and Lecturer and Examiner in Zoology in the University of Manchester. He succeeds the late Professor T. W. Bridge.

Miss Jessie S. Bayliss, D.Sc., a former student, has been appointed Lecturer in Botany.

Walter Myers Travelling Studentship.

Mr. John Dale, M.B., Ch.B., B.Sc., has been recommended for election to the Walter Myers Travelling Studentship, and the Senate has approved of his holding the studentship at Hamburg. This is the first time this scholarship has been awarded since its foundation in 1901.

Matriculation.

The pass list of the September matriculation examination contains the names of fifty-three students, and of these seven have been placed in Class I.

Medico-Legal.

MEDICAL INSPECTION OF SCHOOL CHILDREN.

Etiquette writes: A medical inspector of schools signs a child up as having "a weakness in one lung." This has the effect of greatly disturbing the parents, who bring the child to be examined by their doctor. The doctor finds no symptoms, and no signs of any lung trouble whatsoever, and the parents state that the child has always been perfectly healthy, is gaining weight, and has no cough or trouble of any description. Another medical man examining the child independently agrees with the parent's doctor that there is no lung trouble whatever. What is the course to adopt?

*** A notice given by the medical inspector of schools to a parent is of the nature of a warning, or advice, or recommendation that in his judgement the child requires special care or treatment. The parents in the case related have done their duty in taking the advice of their own doctor with, in addition, the opinion of another medical man. The proper course would seem to be for the parent's doctor to write to the medical inspector, telling him that the child had been carefully examined and appeared free from evidence of disease, and that the parent's doctor had therefore advised them to let the child return to school.

Medico-Ethical.

The advice given in this column for the assistance of members is based on medico-ethical principles generally recognized by the profession, but must not be taken as representing direct findings of the Central Ethical Committee.

THE COURTESY CALL.

Peana.—The courtesy call should be made as soon as possible after the practitioner has settled in a neighbourhood. There can be no reason for deferring it until after the termination of the introduction, unless there is any doubt that he will stop.

Public Health AND POOR LAW MEDICAL SERVICES.

REGULATIONS AS TO IMPORTED MEAT.

It was to be anticipated that the regulations made by the Local Government Board under the Public Health (Regulations as to Food) Act, 1907, would require amendment from time to time as experience in their enforcement was gained. There came into force on September 30th important alterations and additions to the definitions of foreign meat of Class I and Class II. Of special significance is the addition of "stripped" meat to Class I. It appears that the attention of the Board has been called to the fact that meat, particularly beef, is sometimes imported in the form of carcasses, or portions of carcasses, from which the pleura or lining membrane of the thorax have been stripped. The object of this removal of the pleura is no doubt to conceal evidence of disease. When meat in this condition is imported it is now the duty of the Customs Officer to forbid its removal until seen by the medical officer of health.

The fifth annual public meeting of the Association of Women Pharmacists will be held in the lecture theatre of the Pharmaceutical Society at 17, Bloomsbury Square, W.C. (by kind permission of the President and Council), this day, Friday, October 8th, at 8 p.m. An address on The Power of Little Things will be given by Miss Mary Thorne, M.D., F.R.C.S.I.

Letters, Notes, and Answers.

BRITISH MEDICAL ASSOCIATION AND BRITISH MEDICAL JOURNAL.

THE offices of the British Medical Association and of the BRITISH MEDICAL JOURNAL are at 429, Strand, London.

COMMUNICATIONS respecting Editorial matters should be addressed to the Editor, 429, Strand, London, W.C.; those concerning business matters, advertisements, non-delivery of the JOURNAL, etc., should be addressed to the Office, 429, Strand, London, W.C.

TELEGRAPHIC ADDRESS.—The telegraphic address of the EDITOR of the BRITISH MEDICAL JOURNAL is *Aitiology, London*. The telegraphic address of the BRITISH MEDICAL JOURNAL is *Articulate, London*.

TELEPHONE (National):—
2631, Gerrard, EDITOR, BRITISH MEDICAL JOURNAL.
2630, Gerrard, BRITISH MEDICAL ASSOCIATION.
2634, Gerrard, MEDICAL SECRETARY.

☞ *Queries, answers, and communications relating to subjects to which special departments of the* BRITISH MEDICAL JOURNAL *are devoted will be found under their respective headings.*

QUERIES.

A. B. C. asks for information as to the effects of veronal when taken for two months each night, but never more than 7½ grains at a time.

J. E. H. asks whether the bee-sting treatment is likely to be of use in a case diagnosed as rheumatoid arthritis, and, if so, how the treatment can be applied during the winter, when bees are dormant.

NORTHERN CORRESPONDENT asks whether any correspondents have had experience of the beneficial action of exercising the chest muscles for the relief of asthma. It seems to him that such a mode of treatment might be beneficial in other chest troubles as well.

G. P. asks for advice in the treatment of a married lady, aged about 40, who has complained during the past six months of a heavy odour during her periods so perceptible that others, even her little boy, remark it. She wears a pessary, but the condition is thought not to be local. Iron tonics have been tried without effect.

WILLIAM BLAKE'S DRAWINGS.

NORTHERN CORRESPONDENT points out that there is a reference to William Blake in Bulwer Lytton's *A Strange Story*, chapter 64, which may interest Dr. Munro Smith and other correspondents.

ANSWERS.

CONTINUOUS NEURALGIA.

DR. ALBERT E. MORISON (Sunderland) writes: In reply to "Seventy-Two," I have found painting the affected area with guaiacol as frequently as required an almost infallible remedy for this painful condition. After painting it is advisable to cover the part with a thin layer of cotton-wool.

DESTRUCTION OF MOSQUITO LARVAE.

MEDICUS, JUNIOR (India).—The blast-furnace oil recommended by Major Ronald Ross in his book *Mosquito Brigades* could probably be obtained locally, and application might be made to Messrs. Richardson and Cruddas, Bombay, who might be able to supply a waste produot at a cheap rate. Possibly crude rock oils could be obtained from the oil works in Burma, and inquiries might be made of any business agent in Calcutta or Bombay having dealings with Burma. While crude creosote is very good for killing larvae, it is too expensive for practical use; it is better to get any cheap local oil that happens to be available.

SAPROPHYTES AND PARASITES.

A. F.—A saprophyte is an organism which derives its nutriment from non-living material; a parasite is an organism which derives its nutriment from living tissues. The bacteriological use of these terms is not in the least confused, and ought not to be confusing, providing the reader is aware that they are commonly used in a merely descriptive sense, and not as a basis of classification. It might seem perplexing if the same person were described by one man as a blacksmith, and by another as a pew-opener; but the perplexity might be removed by the discovery that the person in question pursued the one vocation on weekdays and the other on Sundays. Similarly, many bacteria are capable of thriving either as saprophytes or as parasites. The typhoid bacillus, for example, is a parasite when it is growing in the spleen or a Peyer's patch, but a saprophyte when growing in the intestinal contents or on the bacteriologist's culture media. Most of the parasitic bacteria—a notable exception being the leprosy bacillus—can be cultivated on artificial media—that is, made to grow as saprophytes. On the other hand, there are many saprophytes which have never been found to grow on living tissues. These are not always harmless. The saprophytes, for example, which would thrive on a blood clot would give rise to decidedly pathogenic effects if the clot containing them happened to be inside the uterus.

LETTERS, NOTES, ETC.

THE SCOTOMAGRAPH.

IN the title of the paper in which Dr. J. H. Tomlinson described his appliance for mapping out blind or anaesthetic areas of the retina and automatically recording the result, published at p. 985 of the issue of the JOURNAL for October 2nd, the term "sectomagraph" was applied to the instrument; its correct designation is Scotomagraph.

THE MECHANISM OF MOTOR FATALITIES.

DR. ARCHIBALD G. HAY (Glasgow) writes: In your paragraph under this heading in the JOURNAL of September 25th, p. 902, reference is made to the need for adjustable foot rests at a proper angle. This, I take it, refers to the back part of the car, as in all modern cars the footboard in front is considerably sloped up. In the Lotis car which I am now using in my practice the manufacturers, Messrs. Sturmey Motors, Ltd., Coventry, supply as a standard fitting a large foot rest at a comfortable angle and adjustable at any desired distance from the seat, so that the need referred to has already been met.

DIABETES AND LECITHIN.

DR. FREDK. WM. ALEXANDER (Blackheath) writes: For the benefit of diabetics I should like to draw attention to the administration of lecithin in the treatment of their malady. I have recommended lecithin with marked results in these cases; after a short time sufferers put on flesh, and experience a feeling of well-being. The fluid form of this phosphorized fat I consider the best for treatment, and to obtain the most successful results it should be taken in a little water half an hour before meals on an empty stomach, so that absorption by the system may take place the more readily. No very hot fluids should be drunk for some time afterwards, that the composition of lecithin may remain unaltered.

THE TREATMENT OF BOILS.

MAJOR G. H. FINK, I.M.S. (ret.), writes: Pure carbolic acid in the treatment of boils is an abortive treatment which followed the treatment recommended some years ago with solid $AgNO_3$ inserted into a boil, and which was fairly successful in certain cases I treated. Prior to this method I had seen a favourite treatment in the North-West Provinces and Oudh among our native assistant surgeons—namely, the painting of the base of a boil, or a crop of newly-formed ones, with solid $AgNO_3$, the object being to counter-irritate and thus disperse, and also to prevent others forming, as we often notice when poultices are used (not *secundum artem*). The prevention of successive crops of boils around the original one is very necessary, and this is possible if the surface around a fairly large area be antiseptically washed, and before applying a poultice to either circumscribe the boil as above with $AgNO_3$, or perhaps better, as it is less painful, to apply a ring of collodion around the base, and over the boil a ring of emplastrum belladonnae with a hole in the centre, of fair size for applying a poultice (either linseed, bread, poppy-heads, charcoal, or any of these in combination, according to the state of the patient). Heat—moist heat particularly—is very essential for the alleviation of pain in the inflammatory stage as well as for maturation; and, whether a poultice be applied or fomentations, the heat must be continuous and not lukewarm in both the stages for a salutary change to result. Steaming the surface of a boil is good. In the summer months, and also in tropical countries, the pure carbolic acid treatment is excellent; but pure carbolic acid mixed with equal parts of camphor will be found not only quite as useful, but less painful. You may insert it hypodermically into the base of the boil with very little pain subsequently. If desired a 10 per cent. solution of cocaine for local anaesthesia might be given in suitable cases. The carbolic-camphor treatment is free from much subsequent pain, and it keeps off flies. While on the subject of boils and their treatment, I should like to mention a note of warning in cases where successive crops appear and cover the body and defy dispersion: examine the urine, and also a thorough examination of the hepatic region in the tropics for tropical abscess. In such cases, in addition to medical treatment, the diet requires careful regulation if there should be the slightest indication of sugar in the urine.

SCALE OF CHARGES FOR ADVERTISEMENTS IN THE BRITISH MEDICAL JOURNAL.

	£	s.	d.
Eight lines and under	0	4	0
Each additional line	0	0	6
A whole column	2	13	4
A page	8	0	0

An average line contains six words.

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NOTE.—It is against the rules of the Post Office to receive *Poste Restante* letters addressed either in initials or numbers.